THE SITUATION IN BIOLOGICAL SCIENCE

Proceedings of the Lenin Academy of Agricultural Sciences of the U.S.S.R.

Session: July 31–August 7, 1948

VERBATIM REPORT

MOSCOW
1949

LENIN ACADEMY OF AGRICULTURAL SCIENCES OF THE U.S.S.R.

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From July 31 to August 7, 1948, the regular Session was held of the Lenin Academy of Agricultural Sciences of the U.S.S.R. The participants in the proceedings of the Session were 47 members of the Academy, scientific workers from agricultural scientific research institutes and experimental stations, professors from agricultural colleges, the biological institutes of the Academy of Sciences of the U.S.S.R. and of the Biological Departments of the Lomonosov Moscow State University, agronomists, zootechnicians, mechanizers and economists. The total number of persons participating in the proceedings of the Session was about 700.

T. D. Lysenko, President of the Lenin Academy of Agricultural Sciences, delivered an address on the situation in biological science.

The participants in the debate on this address were: Academician M. A. Olshansky; Academician J. H. Eichfeld; Academician I. V. Yakushkin; S. I. Isayev, Head of the Fruit and Vegetable Breeding Department of the Saratov Agricultural Institute; Academician N. G. Belenky; Academician P. N. Yakovlev; P. F. Plesetsky, Director of the Ukrainian Scientific Research Institute of Fruit Growing; I. A. Minkevich, Doctor of Agricultural Sciences, Director of the Oil-Bearing Crops Scientific Research Institute of the U.S.S.R.; Professor N. I. Noujdin; N. M. Sisakian, Corresponding Member of the Academy of Sciences of the Armenian S.S.R.; Professor S. G. Petrov; Academician S. S. Perov; Academician V. P. Bushinsky; J. A. Rapoport, Doctor of Biological Sciences; G. A. Babajanyan, Director of the Institute of Plant Genetics of the Academy of Sciences of the Armenian S.S.R.; Academician A. A. Avakian; A. P. Vodkov, Director of the Moscow Plant-Breeding Station; Professor Z. Y. Beletsky; Academician E. I. Ushakova; G. P. Vysokos, Director of the Siberian Scientific Research Institute of Grain Husbandry; I. E. Glushchenko,

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When the debate ended Academician T. D. Lysenko delivered a speech in reply. A comprehensive resolution was adopted on his address.

The participants in the proceedings of the Session addressed a letter of greetings to Comrade J. V. Stalin.

FIRST SITTING

Evening, July 31, 1948

Academician T. D. Lysenko. Comrades, the general meeting of Members of the Lenin Academy of Agricultural Sciences of the U.S.S.R. is hereby declared open.

On behalf of the Ministry of Agriculture of the U.S.S.R. and the Minister, and on behalf of the Lenin Academy of Agricultural Sciences and of myself personally, I welcome the newly confirmed Academicians and wish them success in their work. (*Applause.*)

Our Academy, which bears the great name of V. I. Lenin, must by its work for the benefit of our collective farms and state farms, for the benefit of our Country, justify the deep confidence and the solicitude and attention accorded to us by our Party, our Government and by Comrade Stalin personally. (*Applause.*)

Comrades Academicians, since my address is on the agenda, I would request you to relieve me of the duty of presiding at this session and to elect another chairman to conduct its proceedings. I myself would like to nominate as chairman of this session of the Academy, Academician P. P. Lobanov. (*Applause.*)

If there are any other nominations, please let us have them; if not, we shall consider that P. P. Lobanov is entrusted with the chairmanship of this session. (*Applause.*)

Academician P. P. Lobanov. Comrades, I propose that this session consider one question: The Situation in Biological Science.

Are there any proposals regarding the agenda? There is a motion to endorse the agenda. Are there any other motions?

Voices. No.

Academician P. P. Lobanov. The motion is adopted.

Academician Trofim Denisovich Lysenko, President of the Lenin Academy of Agricultural Sciences of the U.S.S.R., will now deliver his address on the situation in biological science. (*Loud applause.*)

ADDRESS DELIVERED BY ACADEMICIAN T. D. LYSENKO ON THE SITUATION IN BIOLOGICAL SCIENCE

1. BIOLOGY, THE BASIS OF AGRONOMY

Agronomy deals with living bodies—plants, animals, microorganisms. A theoretical grounding in agronomy must, therefore, include knowledge of biological laws. And the more profoundly the science of biology reveals the laws of the life and development of living bodies, the more effective is the science of agronomy.

In essence, the science of agronomy is inseparable from biology. When we speak of the theory of agronomy we mean the discovered and comprehended laws of the life and development of plants, animals, and microorganisms.

The methodological level of biological knowledge, the state of the biological science treating of the laws of the life and development of vegetable and animal forms, i.e., primarily of the science known for half a century now as genetics, is of essential importance for our agricultural science.

2. THE HISTORY OF BIOLOGY: A HISTORY OF IDEOLOGICAL BATTLE

The appearance of Darwin's teaching, expounded in his book, *The Origin of Species*, marked the beginning of scientific biology.

The leading idea of Darwin's theory is the teaching on natural and artificial selection. Selection of variations favourable to the organism has produced, and continues to produce, the fitness which we observe in living nature; in the structure of organisms and their adaptation to their conditions of life. Darwin's theory of selection provided a rational explanation of the fitness observable in living nature. His idea of selection is scientific and true. In substance, his teaching on selection is a sum-

mation of the age-old practical experience of plant and animal breeders who, long before Darwin, produced varieties of plants and breeds of animals by the empirical method.

Darwin investigated the numerous facts obtained by naturalists in living nature and analyzed them through the prism of practical experience. Agricultural practice served Darwin as the material basis for the elaboration of his theory of evolution, which explained the natural causes of the purposiveness we see in the structure of the organic world. That was a great advance in the knowledge of living nature.

In Engels' opinion, three great discoveries enabled man's knowledge of the interconnection of natural processes to advance by leaps and bounds: first, the discovery of the cell; second, the discovery of the transformation of energy; third, "the proof which Darwin first developed in connected form that the stock of organic products of nature environing us today, including mankind, is the result of a long process of evolution from a few originally unicellular germs, and that these again have arisen from protoplasm or albumen, which came into existence by chemical means."[1]

The classics of Marxism, while fully appreciating the significance of the Darwinian theory, pointed out the errors of which Darwin was guilty. Darwin's theory, though unquestionably materialist in its main features, is not free from some serious errors. A major fault, for example, is the fact that, along with the materialist principle, Darwin introduced into his theory of evolution reactionary Malthusian ideas. In our days this major fault is being aggravated by reactionary biologists.

Darwin himself recorded the fact that he accepted the Malthusian idea. In his autobiography we read:

"In October 1838, that is, fifteen months after I had begun my systematic enquiry, I happened to read for amusement *Malthus on Population*, and, being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones

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[1] F. Engels, *Ludwig Feuerbach und der Ausgang der klassischen deutschen Philosophie*, Moskau 1946, S. 44.

to be destroyed.... Here then *I had* at last *got a theory by which to work*."[1] [My emphasis—*T.L.*]

Many are still not clear about Darwin's error in transferring into his teaching Malthus' preposterous reactionary ideas on population. The true scientist cannot and must not overlook the erroneous aspects of Darwin's teaching.

Biologists should always ponder these words of Engels: "The entire Darwinian teaching on the struggle for existence merely transfers from society to the realm of living nature Hobbes' teaching on *bellum omnium contra omnes* and the bourgeois economic teaching on competition, along with Malthus' population theory. After this trick (the absolute justification for which, as indicated in point 1, I deny, particularly in regard to Malthus' theory) has been performed, the same theories are transferred back from organic nature to history and the claim is then made that it has been proved that they have the force of eternal laws of human society. The childishness of this procedure is *obvious,* and it is not worth while wasting words on it. But if I were to dwell on this at greater length, I should have started out by showing that they are poor *economists* first, and only then that they are poor naturalists and philosophers."[2]

For the propaganda of his reactionary ideas Malthus invented an allegedly natural law. "The cause to which I allude," he wrote, "is the constant tendency in all animated life to increase beyond the nourishment prepared for it."[3]

It must be clear to any progressively thinking Darwinist that, even though Darwin accepted Malthus' reactionary theory, it basically contradicts the materialist foundation of his own teaching. Darwin himself, as may be easily noted, being as he was a great naturalist, the founder of scientific biology, whose activity marks an epoch in science, could not be satisfied with the Malthusian theory, since it is, in fact and fundamentally, at variance with the phenomena of living nature.

Under the weight of the vast amount of biological facts accumulated by him, Darwin felt constrained in a number of

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1 *The Life and Letters of Charles Darwin,* London 1887. Vol. I, p. 83.

2 F. Engels, letter to P. L. Lavrov, 12-17 November 1875.

3 T. R. Malthus, *An Essay on the Principle of Population,* London, New York and Melbourne, 1890, Book 1, p. 2.

cases radically to alter the concept of the "struggle for existence," to stretch it to the point of declaring that it was just a figure of speech.

Darwin himself, in his day, was unable to fight free of the theoretical errors of which he was guilty. It was the classics of Marxism that revealed those errors and pointed them out. Today there is absolutely no justification for accepting the erroneous aspects of the Darwinian theory, those based on Malthus' theory of overpopulation with the inference of a struggle presumably going on within species. And it is all the more inadmissible to represent these erroneous aspects as the cornerstone of Darwinism (as I. I. Schmalhausen, B. M. Zavadovsky, and P. M. Zhukovsky do). Such an approach to Darwin's theory prejudices the creative development of its scientific core.

Even when Darwin's teaching first made its appearance, it became clear at once that its scientific, materialist core, the theory of the evolution of living nature, was antagonistic to the idealism that reigned in biology.

Progressively thinking biologists, both in our country and abroad, saw in Darwinism the only right road to the further development of scientific biology. They took it upon themselves to defend Darwinism against the attacks of the reactionaries, with the Church at their head, and of obscurantists in science, such as Bateson.

Such eminent biologists as V. O. Kovalevsky, I. I. Mechnikov, I. M. Sechenov, and particularly K. A. Timiryazev, defended and developed Darwinism with all the passion of true scientists.

K. A. Timiryazev, that great investigator, saw distinctly that only on the basis of Darwinism could the science of the life of plants and animals develop successfully, that only by further developing Darwinism and raising it to new heights would biological science become capable of helping the tiller of the soil to obtain two ears of corn where there was formerly only one.

Darwinism as presented by Darwin contradicted idealistic philosophy, and this contradiction grew deeper with the development of the materialist teaching. Reactionary biologists have therefore done everything in their power to empty Darwinism of its materialist elements. The individual voices of progressive biologists like K. A. Timiryazev were drowned by

the chorus of the anti-Darwinists, the reactionary biologists the world over.

In the post-Darwinian period the overwhelming majority of biologists—far from further developing Darwin's teaching—did all they could to debase Darwinism, to smother its scientific foundation. The most glaring manifestation of such debasement of Darwinism is to be found in the teachings of Weismann, Mendel, and Morgan, the founders of modern reactionary genetics.

3. TWO WORLDS—TWO IDEOLOGIES IN BIOLOGY

Weismannism, which made its appearance at the turn of the century, followed by Mendelism-Morganism, was primarily directed against the materialist foundations of Darwin's theory of evolution.

Weismann named his conception Neo-Darwinism, but, in fact, it was a complete denial of the materialist aspects of Darwinism. It insinuated idealism and metaphysics into biology.

The materialist theory of the evolution of living nature necessarily presupposes the recognition of hereditary transmission of individual characteristics acquired by the organism under definite conditions of its life; it is unthinkable without recognition of the inheritance of acquired characters. Weismann, however, set out to refute this materialist proposition. In his *Lectures on Evolutionary Theory,* he asserts that "not only is there no proof of such a form of heredity, but it is inconceivable theoretically."[1] Referring to earlier statements of his in a similar vein, he declares that "thus war was declared against Lamarck's principle of the direct transforming effect of use and disuse, and, indeed, that marked the beginning of the struggle which is going on to this day, the struggle between the Neo-Lamarckians and the Neo-Darwinians, as the contending parties are called."[2]

Weismann, as we see, speaks of having declared war against Lamarck's principle; but it is easy enough to see that he declared war against that without which there is no

[1] A. Weismann, *Vorträge über Deszendenztheorie,* Bd. 1, Jena 1904, S. 198.

[2] *Ibid.*

materialist theory of evolution, that under the guise of "Neo-Darwinism" he declared war against the materialist foundations of Darwinism.

Weismann denied the inheritability of acquired characters and conceived the idea of a special hereditary substance "to be sought for in the nucleus."[1] *"The sought-for bearer of heredity,"* he stated, *"is contained in the chromosome material."*[2] The chromosomes, he said, contain units, each of which "determines a definite part of the organism in its appearance and final form."[3]

Weismann asserts that there are "two great categories of living material: the *hereditary substance*, or *idioplasm*, and the '*nutrient substance*,' or *trophoplasm*. . . ."[4] He declares that the bearers of the hereditary substance, "the *chromosomes, represent a separate world, as it were*,"[5] a world independent of the body of the organism and its conditions of life.

Having thus disposed of the living body as being merely a nutritive soil for the hereditary substance, Weismann proclaims that the hereditary substance is immortal and is never generated *de novo*.

Thus, he asserts, "the germ-plasm of a species is never generated *de novo*; it only grows and multiplies continually, handed down from generation to generation.... Looked at only from the point of view of propagation, the germ cells are the most important element in the individual specimen, for they alone preserve the species, whereas the body is reduced practically to the status of a mere breeding ground for the germ cells, the place in which they form and, under favourable conditions, feed, multiply, and ripen."[6] The living body and its cells, according to Weismann, are but the *container and nutritive medium* of the hereditary substance; they themselves can never produce the latter, they "can never bring forth germ cells."[7]

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1 A. Weismann, *Vorträge über Deszendenztheorie*, Bd. 1, S. 277.
2 *Ibid.*
3 *Ibid.*, S. 305.
4 *Ibid.*, S. 279.
5 *Ibid.*, S. 239.
6 *Ibid.*, S. 339-40.
7 *Ibid.*, S. 339.

Weismann thus endows the mythical hereditary substance with the property of continued existence; it is a substance which does not itself develop and at the same time determines the development of the mortal body.

Further: "... the hereditary substance of the germ cell, *prior* to the reduction division, potentially contains all the elements of the body."[1] And although Weismann does state that "in the germ-plasm there is no determinant of a 'hooked nose' just as there is no determinant of the wing of a butterfly with all its parts and particles," he goes on to emphasize that, nevertheless, the germ-plasm "... contains a certain number of determinants which successively determine the development of an entire group of cells in all its stages, leading to the formation of the nose in such a mode as to result in a hooked nose, exactly in the same way as the wing of a butterfly, with all its little veins, cells, nerves, trachea, glandular cells, form of scales, and pigment deposits, comes into being by the successive action of multitudinous determinants upon the course of the proliferation of the cells."[2]

Hence, according to Weismann, there can be no new formations of the hereditary substance, it does *not* develop with the development of the individual, and is *not* subject to any dependent changes.

An immortal hereditary substance, independent of the qualitative features attending the development of the living body, directing the mortal body, but not produced by the latter—that is Weismann's frankly idealistic, essentially mystical conception, which he disguised as "Neo-Darwinism."

Weismann's conception has been fully accepted and, we might say, carried further by Mendelism-Morganism.

Morgan, Johannsen, and other pillars of Mendelism-Morganism, declared from the outset that they intended to investigate the phenomena of heredity independently of the Darwinian theory of evolution. Johannsen, for example, wrote in his principal work: "... one of the major aims of our research was to put an end to the harmful dependence of the

[1] A. Weismann, *Vorträge über Deszendenztheorie*, Bd. 1, Jena 1904, S. 282.

[2] *Ibid.*, S. 314.

heredity theories on speculations in the field of evolution."[1] The purpose of the Morganists in making such declarations was to wind up their investigations by assertions which in the final analysis denied evolution in living nature, or recognized it as a process of purely quantitative changes.

I have already said that the conflict between the materialist and the idealist outlook in biological science has been going on throughout its history.

In the present epoch of struggle between two worlds the two opposing and antagonistic trends, penetrating the foundations of nearly all branches of biology, are particularly sharply defined.

Socialist agriculture, the kolkhoz and sovkhoz system, has given rise to a Soviet biological science, founded by Michurin—a science new in principle, developing in close union with agronomic practice, as agronomic biology.

The foundations of Soviet agrobiological science were laid by Michurin and Williams, who generalized and developed the best of what science and practice had accumulated in the past. Their work has enriched our knowledge of the nature of plants and soils, our knowledge of agriculture, with much that is new in principle.

Close contact between science and the practice of collective and state farms creates inexhaustible opportunities for the development of theoretical knowledge, enabling us to learn ever more and more about the nature of living bodies and the soil.

It is no exaggeration to state that Morgan's feeble metaphysical "science" concerning the nature of living bodies can stand no comparison with our effective Michurinist agrobiological science.

The new vigorous trend in biology, or more truly the new Soviet biology, agrobiology, has met with bitter opposition on the part of representatives of reactionary biology abroad, as well as of some scientists in our country.

The representatives of reactionary biological science—Neo-Darwinians, Weismannists, or, which is the same, Mendelist-

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[1] W. Johannsen, *Elemente der exakten Erblichkeitslehre,* Jena 1926, S. 248.

Morganists—uphold the so-called chromosome theory of heredity.

Following Weismann, the Mendelist-Morganists contend that the chromosomes contain a special "hereditary substance" which resides in the body of the organism as though in a case and is transmitted to succeeding generations irrespective of the qualitative features of the body and its conditions of life. The conclusion drawn from this conception is that new tendencies and characteristics acquired by the organism under the influence of the conditions of its life and development are not transmissible and can have no evolutionary significance.

According to this theory, characters acquired by vegetable and animal organisms cannot be handed down, *cannot be inherited.*

The Mendelist-Morganist theory does not include in the scientific concept "living body" the conditions of the body's life. To the Morganists, environment is only the background—indispensable, they admit—for the manifestation and operation of the various characteristics of the living body, in accordance with its heredity. They therefore hold that qualitative variations in the heredity (nature) of living bodies are entirely independent of the environment, of the conditions of life.

The representatives of Neo-Darwinism, the Mendelist-Morganists, hold that the efforts of investigators to regulate the heredity of organisms by suitably changing the conditions of life of these organisms are utterly unscientific. They therefore call the Michurin trend in agrobiology Neo-Lamarckian, which, in their opinion, is absolutely fallacious and unscientific.

Actually, it is the other way round.

First, the well-known Lamarckian propositions, which recognize the active role of external conditions in the formation of the living body and the inheritance of acquired characters, unlike the metaphysics of Neo-Darwinism (or Weismannism), are by no means fallacious. On the contrary, they are quite true and scientific.

Secondly, the Michurin trend cannot be called either Neo-Lamarckian or Neo-Darwinian. It is creative Soviet Darwinism, rejecting the errors of both and free from the defects of the Darwinian theory in so far as it included Malthus' erroneous ideas.

Furthermore, it cannot be denied that in the controversy

that flared up between the Weismannists and Lamarckians in the beginning of the twentieth century, the Lamarckians were closer to the truth; for they defended the interests of science, whereas the Weismannists were at loggerheads with science and prone to indulge in mysticism.

The true ideological content of Morgan's genetics has been well revealed (to the discomfiture of our Morganists) by the physicist Erwin Schrödinger. In his book, *What Is Life? The Physical Aspect of the Living Cell*, he draws some philosophical conclusions from Weismann's chromosome theory, of which he speaks very approvingly. Here is his main conclusion: "...the personal self equals the omnipresent, all-comprehending, eternal self." Schrödinger regards this conclusion as "the closest a biologist can get to proving God and immortality at one stroke."[1]

We, the representatives of the Soviet Michurin trend, contend that inheritance of characters acquired by plants and animals in the process of their development is possible and necessary. Ivan Vladimirovich Michurin mastered these possibilities in his experiments and practical activities. The most important point is that Michurin's teaching, expounded in his works, shows every biologist the way to regulating the nature of vegetable and animal organisms, the way of altering it in a direction required for practical purposes by regulating the conditions of life, i.e., by physiological means.

A sharp controversy, which has divided biologists into two irreconcilable camps, has thus flared up over the old question: *can characters and properties acquired by vegetable and animal organisms in the course of their life be inherited?* In other words, whether qualitative variations of the nature of vegetable and animal organisms depend on the nature of the conditions of life which act upon the living body, upon the organism.

The Michurin teaching, which is in essence materialist and dialectical, proves by facts that such dependence does exist.

The Mendelist-Morganist teaching, which in essence is metaphysical and idealist, denies the existence of such dependence, though it can cite no evidence to prove its point.

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[1] E. Schrödinger, *What Is Life? The Physical Aspect of the Living Cell*, Cambridge University Press, 1945, p. 88.

4. THE SCHOLASTICISM OF MENDELISM-MORGANISM

The chromosome theory is based on Weismann's absurd proposition regarding the continuity of the germ-plasm and its independence of the soma, a proposition which K. A. Timiryazev already condemned. In line with Weismann, the Morganist-Mendelists take it for granted that parents are genetically not the progenitors of their offspring. Parents and children, according to their teaching, are brothers or sisters.

Furthermore, neither parents nor children are really themselves. They are only by-products of the inexhaustible and immortal germ-plasm. Variations in the latter are absolutely independent of its by-product, that is, of the body of the organism.

Let us turn to the Encyclopedia where we naturally may expect to find the quintessence of the question under discussion.

In the 1945 edition of *The Encyclopedia Americana,* T. H. Morgan, founder of the chromosome theory, writes in the article entitled "Heredity": "The germ-cells become later the essential parts of the ovary and testis respectively. In origin, therefore, *they are independent of the rest of the body and have never been a constituent part of it.... Evolution is germinal in origin and not somatic as had been earlier taught.* [My emphasis—*T.L.*] This idea of the origin of new characters is held almost universally to-day by biologists."

The same idea differently worded is propounded in the same *Encyclopedia Americana* by Professor Castle in the article on "Genetics." After stating that usually the organism develops from a fertilized egg, Castle goes on to set forth the "scientific" foundations of genetics as follows:

"In reality the parent does not produce the child nor even the reproductive cell which functions in its origin. The parent is himself merely a byproduct of the fertilized egg (or zygote) out of which he arose. The direct product of the zygote is other reproductive cells, similar to those from which it arose.... Hence heredity (that is, the resemblance between parent and child) depends upon the close connection between the reproductive cells which formed the parent and those which formed the child, one being the immediate and direct product of the other. This principle of the 'continuity of the germinal

substance' (reproductive cell material) is one of the foundation principles of genetics. It shows why body changes produced in a parent by environmental influences are not inherited by the offspring. It is because offspring are not the product of the parent's body but only of the germinal substance which that body harbors. . . . To August Weismann belongs the credit for first making this clear. He may thus be regarded as one of the founders of genetics."

To us it is perfectly clear that the foundation principles of Mendelism-Morganism are false. They do not reflect the reality of living nature and are an example of metaphysics and idealism.

Because this is so obvious, the Mendelist-Morganists of the Soviet Union, though actually fully sharing the principles of Mendelism-Morganism, often conceal them shamefacedly, veil them, conceal their metaphysics and idealism in a verbal shell. They do this because of their fear of being ridiculed by Soviet readers and audiences who are firm in the knowledge that the germs of organisms, or the sex cells, are a result of the vital activity of the parent organisms.

It is only when no mention is made of the fundamentals of Mendelism-Morganism that persons having no detailed knowledge of the life and development of plants and animals can be led to think of the chromosome theory of heredity as a neat system, as in some degree corresponding to the truth. But once we accept the absolutely true and generally known proposition that the reproductive cells, or the germs, of new organisms are produced by the organism, by its body, and not by the very same reproductive cell from which the given, already mature, organism arose, nothing is left of the "neat" chromosome theory of heredity.

Naturally, what has been said above does not imply that we deny the biological role and significance of chromosomes in the development of the cells and of the organism. But it is not at all the role which the Morganists attribute to the chromosomes.

Plenty of examples can be cited to show that our home-grown Mendelist-Morganists accept in its entirety the chromosome theory of heredity, its Weismannist foundations and idealistic conclusions.

Academician N. K. Koltsov, for example, asserts: "Chemically, the genoneme with its genes remains unchanged in the course of the entire ovogenesis and is not subject to metabolism—oxidizing and reduction processes."[1] This assertion, which no literate biologist can accept, denies the existence of metabolism in a section of the living and developing cells. It must be obvious to everyone that N. K. Koltsov's conclusion is fully in line with the Weismannist and Morganist idealist metaphysics.

N. K. Koltsov's false assertion dates back to 1938. It has long since been exposed by the Michurinists, and it would, perhaps, not have been worth while going back to the past if not for the fact that the Morganists persist in holding on to their anti-scientific positions to this day.

We can find further proof of this by turning once more to Schrödinger's book mentioned above. Schrödinger says in substance the same things as Koltsov. Since he shares the idealistic conception of the Morganists, he also asserts that there exists an "*hereditary substance, largely withdrawn from the disorder of heat motion*. . . ."[2] [My emphasis—*T. L.*]

The Russian translator of Schrödinger's book, A. A. Malinovsky (a scientific worker in N. P. Dubinin's laboratory), in his "Postscript" to the book, subscribes—and with good reason —to Haldane's opinion, linking Schrödinger's idea with N. K. Koltsov's views.

In that "Postscript," written in 1947, Malinovsky says: "The view accepted by Schrödinger according to which the chromosome is a gigantic molecule (Schrödinger's 'aperiodic crystal'), was first put forward by the Soviet biologist, Professor N. K. Koltzov, and not by Delbrück, with whose name Schrödinger associates this conception."

There is no point, in this case, in going into the question of who is entitled to claim credit for the authorship of this scholastic view. A more important point is the high appreciation of Schrödinger's book by one of our domestic Morganists, A. A. Malinovsky.

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[1] Н. К. Кольцов, „Структура хромосом и обмен веществ в них", *Биологический журнал*, том VII, вып. I, 1938 г., стр. 42.

[2] E. Schrödinger, *What Is Life? The Physical Aspect of the Living Cell*, p. 85.

Here are a few samples of the praise he showers on this book:

"In a fascinating form, accessible both to the physicist and the biologist, Schrödinger reveals to the reader a new trend rapidly developing in science, a trend largely combining the methods of physics and of biology."

"Strictly speaking, Schrödinger's book represents the first coherent results of this trend.... Schrödinger makes a big contribution of his own to this new trend in the science of life, and this quite justifies the enthusiastic opinions voiced about his book in the foreign scientific press."

Since I am no physicist, I shall say nothing concerning the methods of physics which Schrödinger combines with biology. As for the biology in Schrödinger's book, it is Morganist pure and simple, and this, in fact, is what makes Malinovsky go into raptures over it.

The enthusiastic praise of Schrödinger's book in Malinovsky's "Postscript" speaks eloquently enough of our Morganists' idealistic views and positions.

M. M. Zavadovsky, Professor of Biology in the University of Moscow, writes in an article entitled "The Creative Road of Thomas Hunt Morgan": "Weismann's ideas found a wide response among biologists, and many of them have taken the road suggested by that highly gifted investigator.... Thomas Hunt Morgan was one of those who highly appreciated the main content of Weismann's ideas."[1]

Now what "main content" is meant here?

What is meant is an idea of prime importance to Weismann and all Mendelist-Morganists, including Professor M. M. Zavadovsky. The latter formulates that idea as follows: "What came first, the chicken or the egg? And," writes Professor Zavadovsky, "to this clearly put question Weismann gave an explicit, categorical reply: the egg."[2]

It is obvious to anyone that both the question and the answer which Professor Zavadovsky, following Weismann, gives are nothing but a revival, and a belated one at that, of old scholasticism.

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[1] *Бюллетень Московского общества испытателей природы*, том LII, вып. 3, 1947 г., стр. 86.

[2] *Ibid.*

In 1947 Professor M. M. Zavadovsky repeats and defends the ideas he set forth in 1931 in his work *Dynamics of Development of Organisms*. There M. M. Zavadovsky considered it necessary to "firmly join with *Nussbaum* who maintains that sexual products do not develop from the maternal organism, but from the same source as the latter,"[1] that "the seminal corpuscles and eggs do not originate in the parent organism, but have a common origin with the latter." [2] And in his "General Conclusion" Professor Zavadovsky wrote: "Analysis leads us to the conclusion that the cells of the germ track cannot be regarded as products of somatic tissue. The germ cells and the cells of the soma should be regarded not as daughter and parent generations, but as twin sisters, of which one (the soma) is the feeder, protector, and guardian of the other." [3]

The geneticist, N. P. Dubinin, Professor of Biology, wrote in his article, "Genetics and Neo-Lamarckism": "Genetics quite rightly divides the organism into two distinct sections—the hereditary plasm and the soma. More, this division is one of its foundation principles, one of its major generalizations." [4]

We need not continue the list of authors who, like M. M. Zavadovsky and N. P. Dubinin, frankly expound the ABC of the Morganist system of views. In college textbooks on genetics this ABC is called the "Mendelian laws" (dominance, segregation, purity of gametes, etc.). An example of how uncritically our Mendelist-Morganists accept idealistic genetics is the fact that the standard textbook on genetics in many of our colleges has until quite recently been a translated American, strictly Morganistic, textbook—by Sinnott and Dunn.

Fully in line with the main theses of this textbook, Professor N. P. Dubinin wrote in that same article of his ("Genetics and Neo-Lamarckism"): "Thus the facts of modern genetics rule out any recognition of the 'foundation of foundations' of Lamarckism—*the idea that acquired characters are inherited*." [5] [My emphasis—*T. L.*]

The Mendelist-Morganists have thus thrown overboard one of the greatest acquisitions in the history of biological science

[1] М. Завадовский, *Динамика развития организма*, 1931 г., стр. 321.
[2] *Ibid.*, p. 313.
[3] *Ibid.*, p. 326.
[4] Журнал *Естествознание и Марксизм*, 1929 г., № 4, стр. 83.
[5] *Ibid.*, p. 81.

—the principle of the inheritance of acquired characters, first put forth by Lamarck and subsequently organically incorporated in Darwin's teaching.

To the materialist teaching that it is possible for plants and animals to inherit individual variations of characters acquired under the influence of conditions of life, Mendelism-Morganism opposes an idealistic assertion, dividing the living body into two separate substances: the mortal body (or soma) and an immortal hereditary substance, germ-plasm. It is further categorically maintained that changes in the soma, i.e., in the living body, have no effect whatever upon the hereditary substance.

5. THE IDEA OF UNKNOWABILITY IN THE TEACHING ON "HEREDITARY SUBSTANCE"

Mendelism-Morganism endows the postulated mythical "hereditary substance" with an indefinite variation property. Mutations, i.e., changes of the "hereditary substance," are supposed to have no definite tendency. This assertion of the Morganists is logically connected with the underlying basis of Mendelism-Morganism—the principle that the hereditary substance is independent of the living body and its conditions of life.

The Morganist-Mendelists, who proclaim that hereditary alterations, or "mutations" as they are called, are "indefinite," presume that such alterations *cannot as a matter of principle be predicted*. We have here a peculiar conception of unknowability; its name is idealism in biology.

The assertion that variation is "indefinite" raises a barrier to scientific prediction, thereby handicapping practical agriculture.

Proceeding from the unscientific and reactionary Morganist teaching concerning "indefinite variation," the head of the Department of Darwinism at the University of Moscow, Academician I. I. Schmalhausen, asserts in his *Factors of Evolution* that hereditary variation, in its specific features, does not depend on the conditions of life and therefore has no definite tendency.

"Factors unassimilated by the organism," writes Schmalhausen, "if they reach the organism at all and influence it, can

have but an indefinite effect.... Such influence can only be indefinite. Consequently, all new alterations in the organism, which as yet have no past history, will be indefinite. This category of alterations will include, however, not only mutations as new 'hereditary' changes, but any new (i.e., appearing for the first time) modification."[1]

On a preceding page in the same book Schmalhausen writes: "In the development of any individual, environmental factors perform, in the main, only the role of agents liberating the course of certain form-building processes and the conditions which make it possible to consummate their realization."

This formalistic, autonomistic theory of a "liberating cause" in which the role of external conditions is reduced to the realization of an autonomous process, has long been demolished by the advance of progressive science; it has been exposed by materialism as unscientific in essence, as idealistic.

Schmalhausen and others among our domestic followers of imported Morganism cite Darwin as their authority. In proclaiming the "indefiniteness of variation," they invoke Darwin's statements on the subject. Darwin indeed spoke of "indefinite variability." But that was due to the *limitations* of selection practice in his days. Darwin was aware of that himself and wrote that "we cannot at present explain either the causes or nature of the variability of organic beings." [2] "The subject," he said, "is an obscure one; but it may be useful to probe our ignorance." [3]

The Mendelist-Morganists cling to everything that is obsolete and wrong in Darwin's teaching, at the same time discarding its living materialist core.

In our socialist country, the teaching of the great transformer of nature, I. V. Michurin, has created a fundamentally new basis for directing the variability of living organisms.

Michurin himself and his followers have obtained and are obtaining directed hereditary changes in vegetable organisms

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[1] И. И. Шмальгаузен, *Факторы эволюции*, Изд. АН СССР, 1946 г., стр. 12-13.

[2] Ch. Darwin, *The Variation of Animals and Plants Under Domestication*, Vol. II, London 1885, p. 282.

[3] *Ibid.*, p. 237.

literally in immense quantities. Yet Schmalhausen still asserts that:

"The appearance of individual mutations is by all indications a case of chance phenomena. We can neither predict nor deliberately induce this or that mutation. So far it has been found impossible to establish any causal connection between the quality of mutation and definite changes in the factors of the environment."[1]

On the basis of the Morganist conception of mutations, Schmalhausen has formulated the theory of so-called "stabilizing selection"—a theory profoundly wrong ideologically and hamstringing practical activity. According to Schmalhausen, the formation of breeds and varieties proceeds—presumably inevitably—in a declining curve: the formation of breeds and varieties, rapid at the dawn of culture, increasingly expends its "reserve of mutations" and gradually declines. "Both the formation of breeds of domestic animals and the formation of varieties of cultivated plants," writes Schmalhausen, "proceeded with such exceptional speed mainly, apparently, because of the previously accumulated reserve of variability. Further strictly directed selection is slower. . . ."[2]

Schmalhausen's assertion and his entire conception of "stabilizing selection" follow the Morgan line.

As we know, Michurin, in the course of his lifetime, produced more than 300 new plant varieties. Many of them were produced without sexual hybridization, and all of them were the result of strictly directed selection, including systematic training. It is an insult to progressive science to assert—in face of these facts and subsequent achievements of followers of Michurin's teaching—that strictly directed selection must progressively decline.

Schmalhausen obviously finds that Michurin's facts do not fit in with his theory of "stabilizing selection." In his book, *Factors of Evolution,* he gets out of the difficulty by making no mention of Michurin's work or of the very existence of Michurin as a scientist. Schmalhausen has written a bulky volume on factors of evolution without ever once mentioning—not

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[1] И. И. Шмальгаузен, *Факторы эволюции*, стр. 68.
[2] *Ibid.*, pp. 214-15.

even in his bibliography—either K. A. Timiryazev or I. V. Michurin. Yet Timiryazev bequeathed to Soviet science a remarkable theoretical work bearing practically the same title: *Factors of Organic Evolution.* As for Michurin and the Michurinists, they have put the factors of evolution to work for agriculture, revealed new factors and given us a deeper understanding of the old ones.

Schmalhausen has "forgotten" the Soviet advanced scientists, the founders of Soviet biological science. But at the same time he quotes profusely and repeatedly statements of big and small foreign and native representatives of Morgan's metaphysics and leaders of reactionary biology.

Such is the style of Academician Schmalhausen, who calls himself a "Darwinist." Yet at a meeting of the Faculty of Biology at the University of Moscow his book was recommended as a masterpiece in the creative development of Darwinism. The book has been given a high rating by the deans of the Faculties of Biology at the Universities of Moscow and Leningrad; it has been praised by I. Polyakov, Professor of Darwinism at the University of Kharkov, by the Pro-Rector of the University of Leningrad, Y. Polyansky, by the member of our Academy, B. Zavadovsky, and by other Morganists who sometimes pose as orthodox Darwinists.

6. THE STERILITY OF MORGANISM-MENDELISM

The Morganist-Weismannists, i.e., the adherents of the chromosome theory of heredity, have repeatedly asserted—without any grounds and often in a slanderous manner—that I, as President of the Academy of Agricultural Sciences, have used my office in the interests of the Michurin trend in science, which I share, to repress the other trend, the one opposed to Michurin's.

Unfortunately, so far it has been exactly the other way round, and it is of that that I, as President of the Lenin Academy of Agricultural Sciences, may and should be accused. I have been wanting in strength and ability to make proper use of my official position to create conditions for the more extensive development of the Michurin trend in the various divi-

sions of biological science, and to restrict, if ever so little, the scholastics and metaphysicians of the opposite trend. As a matter of fact, therefore, the trend so far repressed—repressed by the Morganists—happens to be the one which the President represents, namely, the Michurin trend.

We, the Michurinists, must squarely admit that we have hitherto proved unable to make the most of the splendid possibilities created in our country by our Party and the Government for the complete exposure of the Morganist metaphysics, which is in its entirety an importation from foreign reactionary biology hostile to us. It is now up to the Academy, to which a large number of Michurinists have just been added, to tackle this major task. This will be of considerable importance in the matter of training forces and providing more scientific aid to collective farms and state farms.

Morganism-Mendelism (the chromosome theory of heredity) is to this day taught, in a number of versions, in all colleges of biology and agriculture, whereas the study of Michurin genetics has in fact not been introduced at all. In the higher official scientific circles of biologists, too, the followers of Michurin and Williams have often found themselves in the minority. They were a minority in the Lenin Academy of Agricultural Sciences, too. But the situation in the Academy has now sharply changed thanks to the interest taken in it by the Party, the Government, and Comrade *Stalin* personally. A considerable number of Michurinists have been added as members and corresponding members of our Academy, and we expect that more will be added shortly, at the coming elections. This will create a new situation in the Academy and new opportunities for the further development of the Michurin teaching.

There is no truth whatever in the assertion that the chromosome theory of heredity, with its underlying metaphysics and idealism, has hitherto been repressed. The very opposite is the truth.

In our country the practical achievements of the Michurin trend in agrobiological science have been standing in the way of Morganistic cytogenetics.

Aware of the practical worthlessness of the theoretical postulates of their metaphysical "science," and reluctant to give them up and to accept the vigorous Michurin trend, the Morgan-

ists have bent all their efforts to check the development of the Michurin trend which is inherently opposed to their pseudo science.

It is a calumny to assert that somebody has been preventing the cytogenetic trend in biological science from associating itself with practical agriculture in our country. There is no truth whatever in the assertion that "the right to the practical application of the fruits of their labours has been a monopoly of Academician Lysenko and his followers."

The Ministry of Agriculture might tell us exactly what the cytogeneticists have offered for practical application, and, if there have been such offers, whether they were accepted or rejected.

The Ministry of Agriculture might also tell us which of its scientific research institutes (to say nothing of colleges) have not engaged in cytogenetics in general and, particularly, in the polyploidy of plants obtained by the application of colchicine.

I know that many institutes have been engaged and are engaged in this sort of—in my view—scarcely productive activity. More, the Ministry of Agriculture set up a special institution, headed by A. R. Zhebrak, to study questions of polyploidy. I think that this institution, though it has for some years done nothing besides its work on polyploidy, has produced literally nothing of practical value.

Here is one example which might be cited to show how useless is the practical and theoretical program of our domestic Morganist cytogeneticists.

Professor of Genetics, N. P. Dubinin, Corresponding Member of the Academy of Sciences of the U.S.S.R., who is regarded by our Morganists as the most eminent among them, has worked for many years to ascertain the differences in the cell nuclei of fruit flies in urban and rural localities.

For the sake of complete clarity, let us mention the following. What Dubinin is investigating is not qualitative alterations —in this case, in the nucleus of the cell—resulting from the action of qualitatively different conditions of life. What he is studying is not the inheritance of characteristics acquired by fruit flies under the influence of definite conditions of life, but changes, recognizable in the chromosomes, in the composition

of the population of these flies as the result of the simple destruction of a part of them, for one thing, during the war. Dubinin and other Morganists call such destruction "selection." (*Amusement.*) It is this sort of "selection," identical with an ordinary sieve, which has nothing in common with the truly creative role of selection, that constitutes the subject of Dubinin's investigations.

His work is entitled: "Structural Variability of Chromosomes in Populations of Urban and Rural Localities."

Here are a few quotations from it:

"During the study of various populations of *D. funebris* in the work of 1937 the fact was noted that there were noticeable differences as regards concentration of inversions. Tinyakov stressed this phenomenon on the basis of extensive material. However, only the 1944-45 analysis has shown us that these substantial differences are due to the differences of conditions of habitat in town and in countryside.

"The population of Moscow has eight different orders of genes. In the second chromosome there are four orders (one standard and three different inversions). One inversion in the III chromosome and one in IV ... Inv. II—1 has its limits from 23 C to 31 B. Inv. II—2, from 29 A to 32 B. Inv. II—3, from 32 B to 34 C. Inv. III—1, from 50 A to 56 A. Inv. IV—1, from 67 C to 73 A/B. In the course of 1943-45 the karyotype of 3,315 individuals in the population of Moscow was studied. The population contained immense concentrations of inversions, which proved to be different in various sections of Moscow." [1]

Dubinin went on with his investigations during and after the war and studied the problem of the fruit flies in the city of Voronezh and its environs. He writes:

"The destruction of industrial centres during the war upset the normal conditions of life. The *Drosophila* populations found themselves in severe conditions of existence which, possibly, surpassed the severity of wintering in rural localities. It was of profound interest to study the influence of the changes in the conditions of existence caused by the war upon the karyotypical structure of urban populations. In the spring of 1945 we studied populations from the city of Voronezh, one of the cities

[1] *Доклады АН СССР*, 1946 г., том LI, № 2, стр. 152.

that suffered the worst destruction as the result of the German invasion. Among 225 individuals only two flies were found to be heterozygous for inversion II—2 (0.88%). Thus the concentration of inversions in this large city proved to be lower than in some rural localities. We see here the disastrous influence of natural selection upon the karyotypical structure of the population."[1]

Dubinin, as we see, writes so that outwardly his work may appear to some to be even scientific. As a matter of fact, this was one of the main works on the basis of which Dubinin was elected Corresponding Member of the Academy of Sciences of the U.S.S.R.

But if we were to put it all in plainer terms, stripping it of the pseudoscientific verbiage and replacing the Morganist jargon with ordinary Russian words, we would arrive at the following:

As the result of many years of effort Dubinin "enriched" science with the "discovery" that during the war there occurred among the fruit-fly population of the city of Voronezh and its environs an increase in the percentage of flies with certain chromosome structures and a decrease in the percentage of flies with other chromosome structures (in the Morganist jargon that is called "concentration of inversions" II—2).

Dubinin is not content with these discoveries, "highly valuable" from the theoretical and practical standpoint, which he made during the war. He sets himself further tasks for the restoration period. He writes:

"It will be very interesting to study in the course of several coming years the restoration of the karyotypical structure of the urban population in connection with the restoration of normal conditions of life."[2] (*Animation. Laughter.*)

That is typical of the Morganists' "contribution" to science and practical activity before the war and during the war, and those are the vistas of the Morganist "science" for the restoration period! (*Applause.*)

[1] *Ibid.*, p. 153.
[2] *Ibid.*

7. MICHURIN'S TEACHING, THE FOUNDATION OF SCIENTIFIC BIOLOGY

Contrary to Mendelism-Morganism, with its assertion that the causes of variation in the nature of organisms are unknowable and its denial that directed changes in the nature of plants and animals are possible, I. V. Michurin's motto was: "We cannot wait for favours from Nature; we must wrest them from her."

His studies and investigations led I. V. Michurin to the following important conclusion: "It is possible, with man's intervention, to *force* any form of animal or plant *to change more quickly and in a direction desirable to man.* There opens before man a broad field of activity of the greatest value to him."[1]

The Michurin teaching flatly rejects the fundamental principle of Mendelism-Morganism that heredity is completely independent of the plants' or animals' conditions of life. The Michurin teaching does not recognize the existence in the organism of a separate hereditary substance which is independent of the body. Changes in the heredity of an organism or in the heredity of any part of its body are the result of changes in the living body itself. And changes of the living body occur as the result of departure from the normal in the type of assimilation and dissimilation, of departure from the normal in the type of metabolism. Changes in organisms or in their separate organs or characters may not always, or not fully, be transmitted to the offspring, but changed germs of newly generated organisms always occur only as the result of changes in the body of the parent organism, as the result of direct or indirect action of the conditions of life upon the development of the organism or its separate parts, among them the sexual or vegetative germs. Changes in heredity, acquisition of new characters and their augmentation and accumulation in successive generations are always determined by the organism's conditions of life. Heredity changes and its complexity increases as the result of the accumulation of new characters and properties acquired by organisms in successive generations.

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[1] И. В. Мичурин, *Сочинения*, том IV, стр. 72.

The organism and the conditions required for its life constitute a unity. Different living bodies require different environmental conditions for their development. By studying the character of these requirements we come to know the qualitative features of the nature of organisms, the qualitative features of heredity. Heredity is *the property of a living body to require definite conditions for its life and development and to respond in a definite way to various conditions.*

Knowledge of the natural requirements of an organism and its response to external conditions makes it possible to govern the life and development of the organism. By regulating the conditions of life and development of plants and animals we can probe their nature ever more deeply and thus establish what are the means of changing it in the required direction. Once we know the means of regulating development we can change the heredity of organisms in a definite direction.

Each living body builds itself out of the conditions of its environment after its own fashion, according to its heredity. That is why different organisms live and develop in the same environment. As a rule, each given generation of a plant or animal develops largely in the same way as its predecessors, particularly its close predecessors. *Reproduction of beings similar to itself is a general characteristic of every living body.*

When an organism finds in its environment the conditions suitable to its heredity, its development proceeds in the same way as it proceeded in previous generations. When, however, organisms do not find the conditions they require and are forced to assimilate environmental conditions which, to some degree or other, do not accord with their nature, then the organisms or sections of their bodies become more or less different from the preceding generation. If the altered section of the body is the starting point for the new generation, the latter will, to some extent or other, differ from the preceding generations in its requirements and nature.

The cause of changes in the nature of a living body is a change in the type of assimilation, in the type of metabolism. For example, the process of vernalization (yarovization) of spring cereals does not require lowered temperatures. Normally it proceeds in temperatures such as obtain in the spring and summer in the fields. But by using lower temperature condi-

tions in the vernalization of spring cereals it is possible, after two or three generations, to turn them into winter cereals. And winter cereals cannot pass through the process of vernalization without lowered temperatures. Here is a concrete example showing how a new requirement is induced in the offspring of these particular plants—the requirement for lowered temperatures as a condition for vernalization.

Sex cells and any other cells through which organisms propagate are produced as the result of the development of the whole organism, by means of conversion, by means of metabolism. The phases in the development of an organism are accumulated, as it were, in the cells from which the new generation originates.

We may therefore say that to the extent to which in the new generation the body of an organism (a plant, say) is built anew to that same extent also all its properties, including heredity, develop.

In one and the same organism the development of different cells and of different parts of cells, the development of individual processes, requires different external conditions.

Besides, these conditions are assimilated in different ways. It should be stressed that in this case we mean by *external* that which *is assimilated,* and by *internal* that which *assimilates.*

The life of an organism proceeds through innumerable correlated processes and conversions. The food that enters the organism from the external environment undergoes a series of conversions whereby it is assimilated by the living body, changing from external to internal. This internal, since it is living matter, enters into metabolic relations with the substances of other cells and particles of the body, feeding them and thus becoming external with regard to them.

Two kinds of qualitative changes are observed in the development of vegetable organisms.

1. Changes connected with the process of the realization of the individual cycle of development, when natural requirements, i.e., heredity, are normally met by the *corresponding external conditions.* The result is a body of the same breed and heredity as the preceding generations.

2. Changes in the nature of the organisms, i.e., the changes in heredity. Such changes are also the result of individual

development, but deviating from the normal, usual course. Changes in heredity are as a rule the result of the organism's development *under external conditions which, to one extent or other, do not correspond to the natural requirements of the given organic form.*

Changes in the conditions of life make the very type of development of vegetable organisms change. A changed type of development is thus the primary cause of changes in heredity. Organisms which cannot change in accordance with the changed conditions of life do not survive, leave no progeny.

Organisms, and hence also their nature, are created only in the process of development. Of course, a living body may undergo an alteration also outside the process of development (a burn, a break in joints, in roots, etc.), but such alterations will not be characteristic or necessary for the vital process.

Numerous facts go to show that changes in various sections of the body of a vegetable or animal organism are not fixed by the reproductive cells with the same frequency or to the same extent.

This is explained by the fact that the process of development of each organ, of each particle of the living body, requires relatively definite external conditions. These conditions are selected from the environment by the development of each organ and minutest organule. Therefore, if a section of the body of a vegetable organism is forced to assimilate conditions relatively unusual for it and as a result undergoes alteration and becomes different from the analogous section of the body in the preceding generation, the substances which it sends forth to neighbouring cells may not be selected by the latter, may not be joined into the further chain of corresponding processes. Of course, there will still be a connection between the altered section of the vegetable organism and the other sections of the body, for otherwise it could not exist at all; but this connection may not be fully reciprocal. The altered section of the body will be receiving this or that food from the neighbouring sections; but it will not be able to give away its own specific substances, because the neighbouring sections will refuse to select them.

This explains the frequently observed phenomenon when altered organs, characters, or properties of an organism do not

appear in the progeny. But the altered sections of the body of the parent organism always possess an altered heredity. Fruit growers and horticulturists have long known these facts. An altered twig or bud of a fruit tree or the eye (bud) of a potato tuber cannot as a rule influence the alteration of heredity of the offspring of the given tree or tuber which are not directly generated from the altered sections of the parent organism. If, however, the altered part is cut away and grown separately as an independent plant, the latter, as a rule, will possess a changed heredity, the one that characterized the altered part of the parent body.

The extent of the hereditary transmission of alterations depends on the extent to which the substances of the altered section of the body join in the general process which leads to the formation of reproductive sex or vegetative cells.

Once we know how the heredity of an organism is built up, we can change it in a definite direction by creating definite conditions at a definite moment in the development of the organism.

Good varieties of plants or animals are always produced only by the application of proper methods of cultivation or breeding. Under poor cultivation no good varieties can ever be produced out of poor ones, and in many cases even good cultivated varieties will deteriorate after a few generations. It is a basic rule in seed growing that plants grown for seed must be tended with the utmost care. They must be provided with conditions meeting the optimum of the hereditary requirements of the given plants. Of well-cultivated plants the very best are selected for seed. That is the way varieties of plants are improved in practice. Under poor cultivation, no selection of the best plants for seed will produce the required results—all the seeds obtained will be poor, and the best among them will still be poor.

According to the chromosome theory of heredity, hybrids can only be produced by sexual reproduction. That theory denies the possibility of obtaining vegetative hybrids, for it denies that the conditions of life have any specific influence upon the nature of plants. I. V. Michurin, on the other hand, not only recognized the possibility of producing vegetative hybrids, but elaborated the "mentor" method. This method consists in the following: by grafting cuttings (twigs) of old varieties of fruit trees on the branches of a young variety, the latter ac-

quires properties which it lacks, these properties being transmitted to it through the grafted twigs of the old variety. That is why I. V. Michurin called this method "mentor." The stock is also used as a mentor. By this method Michurin produced new and improved existing varieties.

I. V. Michurin and the Michurinists have found methods of obtaining vegetative hybrids in large quantities.

The vegetative hybrids are cogent proof that Michurin's conception of heredity is correct. At the same time they represent an insuperable obstacle to the theory of the Mendelist-Morganists.

Organisms grafted before they have reached the phase of full formation, i.e., before they have completed their cycle of development, will always undergo changes of development as compared with plants which have their own roots, i.e., ungrafted plants. In the union of plants by means of grafting the product is a single organism with varying breed, that of the scion and that of the stock. By planting the seeds from the scion or the stock it is possible to obtain offspring, individual representatives of which will possess the characteristics not only of the breed from which the seed has been taken, but also of the other with which it has been united by grafting.

Obviously, the stock and the scion could not have exchanged chromosomes of the cell nuclei; yet inherited characters have been transmitted from stock to scion and vice versa. Consequently, *the plastic substances produced by the scion and the stock possess the characters of the breed,* are endowed with definite heredity *just as the chromosomes, and just as any particle of the living body.*

Any character may be transmitted from the one breed to another by means of grafting just as well as by the sexual method.

The wealth of factual material concerning vegetative transmission of various properties of potatoes, tomatoes, and a number of other plants leads us to the conclusion that vegetative hybrids do not differ in principle from sexual hybrids.

The representatives of Mendel-Morgan genetics are not only unable to obtain alterations of heredity in a definite direction, but categorically deny that it is possible to change heredity so as adequately to meet the action of environmental conditions. The principles of Michurin's teaching, on the other hand, tell

us that it is possible to obtain changes in heredity *fully corresponding to the effect of the action of conditions of life.*

A case in point is the experiments to convert spring forms of bread grains into winter forms, and winter forms into still hardier ones in regions of Siberia, for example, where the winters are severe. These experiments are not only of theoretical interest. They are of considerable practical value for the production of frost-resistant varieties. We already have winter forms of wheat obtained from spring forms, which are not inferior, as regards frost-resistance, to the most frost-resistant varieties known in practical farming. Some are even superior.

Many experiments show that when an old established property of heredity is being eliminated, we do not at once get a fully established, solidified new heredity. In the vast majority of cases, what we get is an organism with a plastic nature, which I. V. Michurin called "destabilized."

Vegetable organisms with a "destabilized" nature are those in which their conservatism has been eliminated, and their selectivity with regard to external conditions is weakened. Instead of conservative heredity, such plants preserve, or there appears in them, only a *tendency* to show some preference for certain conditions.

The nature of a vegetable organism may be destabilized:

1. By grafting, i.e., by uniting the tissues of plants of different breeds;

2. By bringing external conditions to bear upon it at definite moments, when the organism undergoes this or that process of its development;

3. By crossbreeding, particularly of forms sharply differing in habitat or origin.

The best biologists, first and foremost I. V. Michurin, have devoted a great deal of attention to the practical value of vegetable organisms with destabilized heredity. Plastic vegetable forms with unestablished heredity, obtained by any of the enumerated methods, should be further bred from generation to generation in those conditions, the requirement of which, or adaptability to which, we want to induce and perpetuate in the given organisms.

In most vegetable and animal forms new generations develop only after fertilization—the fusion of female and male repro-

ductive cells. The biological significance of the process of fertilization is that thereby organisms are produced with a dual heredity—maternal and paternal. Dual heredity lends vitality to organisms and widens the range of their adaptability to varying conditions of life.

It is the usefulness of enriching heredity that determines the biological necessity for crossbreeding forms differing from each other even if ever so slightly.

The vitality of vegetable forms may be renovated and strengthened also by the vegetative, asexual method. This is brought about by the living body assimilating new external conditions, conditions unusual for it. In experiments in vegetative hybridization, in experiments with the aim of producing spring forms from winter forms or vice versa, and in a number of other cases of the nature of organisms being destabilized, we may observe the renovation and strengthening of the vitality of organisms.

By regulating external conditions, the conditions of life of vegetable organisms, we can change varieties in a definite direction and create varieties with desirable heredity.

Heredity is the effect of the concentration of the action of environmental conditions assimilated by the organism in a series of preceding generations.

By means of skilful hybridization, by the method of sexual conjugation of breeds, it is possible at once to unite in one organism that which has been assimilated and solidified in the crossed breeds by many generations. But, according to Michurin's teaching, no hybridization will produce the desired results, unless the conditions are created which will promote the development of the characters which we want the newly-bred or improved variety to inherit.

I have here propounded Michurin's teaching in most general outline. The important point that must be stressed here is that it is absolutely necessary for all Soviet biologists to make a profound study of this teaching. The best way for scientific workers in various branches of biology to master the theoretical depths of the Michurin teaching is to study Michurin's works, to read them over again and again, and to analyze them with a view to solving problems of practical importance.

Socialist agriculture stands in need of a developed, profound biological theory which will help us quickly and properly to perfect the methods of cultivating plants and obtaining plentiful crops and stable yields. It stands in need of a profound biological theory which will help workers in agriculture to obtain in a short time the highly productive forms of plants they need, to correspond to the high fertility which the collective farmers are creating on their fields.

Unity of theory and practice—that is the highroad for Soviet science. The Michurin teaching best embodies this unity in biological science.

In my speeches and writings I have cited numerous examples of the successful application of the Michurin teaching in solving questions of practical importance in various departments of plant breeding. Here I shall take the liberty to dwell briefly on some questions of animal breeding.

As in the case of vegetable forms, the development of animal forms is closely linked with their conditions of life, with the conditions of their environment.

The basic factors for increasing the productivity of domestic animals, for improving existing breeds and producing new ones, are their food and the conditions in which they are kept. This is particularly important if the effectiveness of crossbreeding is to be heightened. Various breeds of domestic animals have been and are produced by men for various purposes and under various conditions. Each breed therefore requires its own conditions of life, those that contributed to its formation.

The greater the divergences between the biological properties of a breed and the conditions of life provided for the individual animals, the less will be the economic value of the given breed.

For example, the advantages—from an economic standpoint—of rich pastures and good feeding with succulent and concentrated fodders are smaller in the case of cattle which by nature cannot give much milk than in the case of cattle with high milking capacities. In the former case we obviously have a breed which, in the economic respect, does not justify the conditions provided for it. Such a breed should be improved by crossbreeding so as to adjust it to the conditions of feeding and maintenance.

On the other hand, a breed noted for its milk-yielding properties, when placed in conditions of poor feeding and maintenance, will not only fail to live up to its reputation as a milk producer, but its chances of survival will be diminished. In such cases the conditions of feeding and maintenance should be improved so as to adjust them to the breed.

Our science and practice of animal breeding, in line with the state plan for obtaining produce in the required quantities and of proper quality, must be guided by the principle: *to select and improve breeds in accordance with the conditions of feeding, maintenance and climate, and at the same time to create conditions of feeding and maintenance most suitable to the given breeds.*

The principal method of constantly improving breeds is to select pedigreed animals best suited for the required aim and at the same time to improve the conditions of feeding, maintenance and care that are most conducive to the development of the animals in the desired direction.

Crossbreeding is a radical and quick method of changing breeds, that is to say, the progeny of the given animals.

In crossbreeding we get, as it were, a union of two breeds evolved by man in the course of a long period of time by creating various conditions of life for the animals. But the nature (heredity) of crosses, particularly in the first generation, is usually unstable and easily responds to the action of the conditions of life, feeding, and maintenance.

Therefore, in crossbreeding it is of especial importance, when choosing a breed for the improvement of a local breed, to bear in mind the conditions of feeding, maintenance, and climate. At the same time, in order to develop the characters and properties which we want to induce in the local breed by crossbreeding, we must provide conditions of feeding and maintenance conducive to the development of the new improving breed properties; otherwise, we may fail to establish the desired qualities and the local breed may even lose some of its good qualities.

I have given an example of the application of the general principles of the Michurin teaching to animal husbandry to show that Soviet Michurin genetics, revealing as it does the general laws of the development of living bodies in order to

cope with problems of practical importance, is also applicable to stockbreeding.

When we speak of mastering the teaching of Michurin we also mean the development and deepening of this teaching, the development of scientific biology. That is the line along which we must secure the growth of the forces of our Michurinist biologists so as to provide ever increasing scientific assistance to the collective farms and state farms in coping with the tasks set by the Party and the Government. (*Applause.*)

8. YOUNG SOVIET BIOLOGISTS SHOULD STUDY THE MICHURIN TEACHING

Unfortunately, so far the Michurin science has not been taught in our universities and colleges. We Michurinists are greatly to blame for this. But it will be no mistake to say that it is also the fault of the Ministry of Agriculture and the Ministry of Higher Education.

To this day Morganism-Mendelism is taught in the majority of our universities and colleges in the departments of genetics and selection, and in many cases also in the departments of Darwinism, whereas the Michurin teaching, the Michurin trend in science, fostered by the Bolshevik Party and by Soviet reality, remains in the shade.

The same may be said of the position with regard to the training of young scientists. By way of illustration, we shall cite the following. In an article "On Doctors' Theses and the Responsibility of Opponents," printed in issue No. 4 of the *Vestnik Vysshey Shkoly* (*Higher Education Messenger*) for 1945, Academician P. M. Zhukovsky, who is the Chairman of the Biology Experts' Commission under the Highest Committee on Academic Degrees, wrote: "A deplorable situation has developed in the matter of theses on genetics. *Theses on genetics are very rare*; they represent, in fact, solitary instances. This is to be explained by the abnormal relations, which have assumed the character of enmity, between the adherents of the chromosome theory of heredity and its opponents. The truth of the matter is that the former somewhat fear the latter, who are very aggressive in their polemics. It would be better to put an

end to this situation. Neither the Party nor the Government forbid the chromosome theory of heredity, and it is freely propounded in universities and colleges. So let the controversy go on."[1]

Let us first note that P. M. Zhukovsky confirms that the chromosome theory of heredity is freely taught in universities and colleges. That is true. But he wants more: he wants Mendelism-Morganism to be still more widely propounded in our colleges. He wants us to have many more Mendelist-Morganist Masters and Doctors of Science who would still more extensively propagate Mendelism-Morganism in our universities and colleges. That, in fact, is what Academician Zhukovsky is driving at in a large section of his article, and that reflects his general line as Chairman of the Biology Commission.

No wonder therefore that the Commission set up all sorts of obstacles in the case of theses on genetics whose authors attempted, even if ever so timidly, to develop this or that principle of Michurin genetics. On the other hand, theses by Morganists, enjoying P. M. Zhukovsky's patronage, appeared and were passed on favourably not at all so rarely—in any event, oftener than the interests of true science required. True enough, theses with a Morganist tendency appeared more rarely than Academician P. M. Zhukovsky would have liked. But there are reasons for this. Under the influence of the Michurin criticism of Morganism young scientists with philosophical training have in recent years come to realize that the Morganist views are utterly alien to the world outlook of Soviet people. In this light the position of Academician P. M. Zhukovsky is rather dubious, seeing that he advises young biologists to pay no heed to the Michurinists' criticism of Morganism, but to go on developing the latter.

Soviet biologists are right when they are suspicious of the Morganist views and refuse to listen to the scholasticism of the chromosome theory. They stand to gain, always and in everything, if they will ponder more often on what Michurin said of this scholasticism.

I. V. Michurin held that Mendelism "...contradicts the truths of nature, before which no artful structure reared out

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[1] *Вестник высшей школы*, № 4, 1945 г., стр. 30.

of wrongly understood phenomena can stand up." "What I would like," he wrote, "is that the thinking unbiased observer should ponder over this and personally test the truth of these conclusions; they represent a basis which we bequeath to naturalists of coming centuries and milleniums."[1]

9. FOR A CREATIVE SCIENTIFIC BIOLOGY

I. V. Michurin laid the foundations for the science of regulating the nature of plants. These foundations have wrought a change in the very method of thinking when dealing with problems of biology.

A knowledge of *causal* connections is essential for the practical work of regulating the development of cultivated plants and domestic animals. For biological science to be in a position to render the collective and state farms ever greater assistance in obtaining higher crop yields, higher yields of milk, etc., it must comprehend the complex biological interrelations, the laws of the life and development of plants and animals.

A scientific handling of practical problems is the surest way to a deeper knowledge of the laws of development of living nature.

Biologists have paid very little attention to the study of the interrelations, the natural and historical connections that exist between individual bodies, individual phenomena, parts of individual bodies and links of individual phenomena. Yet only these connections, interrelations, and natural interactions enable us to understand the process of development, the essence of biological phenomena.

But when living nature is studied in isolation from practical activity the scientific principle of the study of biological connections is lost.

The Michurinists, in their investigations, take the Darwinian theory of evolution as their basis. But in itself Darwin's theory is absolutely insufficient for dealing with the practical problems of socialist agriculture. That is why the basis of contemporary Soviet agrobiology is Darwinism transformed in

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[1] И. В. Мичурин, *Сочинения*, том III, стр. 308-09.

the light of the teachings of Michurin and Williams and thereby converted into Soviet creative Darwinism.

Many problems of Darwinism assume a different aspect as the result of the development of our Soviet agrobiological science, of the Michurin trend in agrobiology. Darwinism has not only been purified of its deficiencies and errors and raised to a higher level, but has undergone a considerable change in a number of its principles. From a science which primarily *explains* the past history of the organic world, it is becoming a creative, *effective means* of systematically mastering living nature, making it serve practical requirements.

Our Soviet Michurinist Darwinism is a creative Darwinism which poses and solves problems of the theory of evolution in a new way, in the light of Michurin's teaching.

I cannot in this report touch on many of the theoretical problems of great practical significance.

I shall dwell briefly on only one of them—namely, the question of intra- and interspecific relations in living nature.

The time has come to consider the question of speciation, approaching it from the angle of the transition of quantitative accumulation into qualitative specific distinctions.

We must realize that speciation is a transition—in the course of the historical process—from quantitative to qualitative variations. Such a leap is prepared by the vital activity of organic forms themselves, as the result of quantitative accumulations of responses to the action of definite conditions of life, and that is something that can definitely be studied and directed.

Such an understanding of speciation, an understanding of natural laws, places in the hands of biologists a powerful means of regulating the vital process itself and consequently speciation as well.

I think that in posing the question this way we may assume that what leads to the appearance of a new specific form, to the formation of a new species out of an old one, is not the accumulation of quantitative distinctions by which varieties within a species are usually recognized. The quantitative accumulations of variations which lead to the leap which changes an old form of species into a new form are variations of a *different order*.

Species are not an abstraction, but actually existing links in the general biological chain.

Living nature is a biological chain broken up, as it were, into individual links or species. It is therefore wrong to say that a species does not retain the constancy of its qualitative definiteness as a species for any length of time. To insist on that would be to regard the evolution of living nature as proceeding as if along a plane, without any leaps.

I am confirmed in this opinion by the data of experiments for the conversion of hard wheat (durum) into soft (vulgare).

Let me note that all systematists admit that these are good, indisputable, independent species.

We know that there are no true winter forms among hard wheats, and that is why in all regions with a relatively severe winter hard wheat is cultivated only as a spring, not a winter, crop. Michurinists have mastered a good method of converting spring into winter wheat. It has already been mentioned that many spring wheats have been experimentally converted into winter wheat. But all of those belonged to the species of soft wheat. When experiments were started to convert hard wheat into winter wheat it was found that after two, three or four years of autumn planting (required to turn a spring into a winter crop) durum becomes vulgare, that is to say, one species is converted into another. Durum wheat with 28 chromosomes is converted into several varieties of soft 42-chromosome wheat, nor do we, in this case, find any transitional forms between the durum and vulgare species. *The conversion of one species into another takes place by a leap.*

We thus see that the formation of a new species is prepared by an alteration of vital activity under definite new conditions in a number of generations. In our case it is necessary to bring autumn and winter conditions to bear on hard wheat in the course of two, three or four generations. Then it can change by a leap into soft wheat without any transitional forms between the two species.

I think that it may be pertinent to note that what led me to study the essentially theoretical problems of species and of intraspecific and interspecific relations among individuals, was never mere curiosity or a fondness for abstract theorizing. I was

and am led to study these questions of theory by my work in the course of which I have to find answers to purely practical problems. For a correct understanding of the relations among individuals within a species and between species it was necessary to have a clear idea of the qualitative distinctions of intraspecific and interspecific diversities of forms.

It thus became possible to find new solutions to such problems of practical importance as weed control in farming, or the choosing of ingredients for the sowing of grass mixtures, or the speedy and extensive afforestation of steppe areas, and many others.

That is what led me to make a new study of the problem of intra- and interspecific struggle and competition, and after a thorough and comprehensive investigation I have come to the conclusion that there exist no intraspecific struggle and mutual assistance among individuals within a species, and that there does exist interspecific struggle and competition and also mutual assistance between different species. I regret that I have so far done very little to elucidate the theoretical implications and practical significance of these questions in the press.

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I shall now conclude. Thus, Comrades, as regards the theoretical line in biology, Soviet biologists hold that the Michurin principles are the only scientific principles. The Weismannists and their followers, who deny the heritability of acquired characters, are not worth dwelling on at too great length. The future belongs to Michurin. (*Applause.*)

V. I. Lenin and *J. V. Stalin* discovered I. V. Michurin and made his teaching the possession of the Soviet people. By their great paternal attention to his work they saved for biology the remarkable Michurin teaching. The Party, the Government, and *J. V. Stalin* personally, have taken an unflagging interest in the further development of the Michurin teaching. There is no more honourable task for us Soviet biologists than creatively to develop Michurin's teaching and to follow in all our activities Michurin's style in the investigation of the nature of the development of living beings.

Our Academy must work to develop the Michurin teaching. In this it ought to follow the personal example of concern for

the work of I. V. Michurin shown by our great teachers—*V. I. Lenin* and *J. V. Stalin*. (*Loud applause.*)

Academician P. P. Lobanov. There will be no sitting on August 1. The participants at the session are invited tomorrow to visit the Experimental Base of the Academy at Gorki-Leninskiye and acquaint themselves with the researches being conducted there.

Academician V. P. Mosolov has the floor for an announcement.

Academician V. P. Mosolov. Those who desire to take part in the excursion to Gorki-Leninskiye should meet tomorrow at 11 in the morning at the Lenin Academy of Agricultural Sciences. They will be taken to Gorki-Leninskiye by bus.

SECOND SITTING

Morning, August 2, 1948

Academician P. P. Lobanov. Comrades, with your permission we shall continue the work of the session.

There is a motion to adopt the following standing order: morning sitting to be held from 11 a.m. to 3 p.m.; dinner recess from 3 p.m. to 6 p.m.; evening sitting from 6 p.m. to 10 p.m. Are there any other motions regarding standing orders? (None.) Are there any objections? (None.) Adopted.

There is a motion to fix the time limit for speakers at thirty minutes. Are there any objections? (None.) Adopted.

We now pass to the discussion of the address. Academician M. A. Olshansky has the floor.

Academician M. A. Olshansky. When judging the correctness of one or another theory, it is important to ascertain to what extent that theory assists practical work. If the Michurin doctrine and Mendelism-Morganism are compared from the standpoint of plant-breeding and seed-growing practice, it will be seen with absolute clarity that Mendelism-Morganism, far from being an assistance, is often a direct hindrance to practice. Michurin genetics, on the other hand, arms plant breeders with effective methods of improving the breed qualities of seed. I shall give a few examples.

Only ten years ago seed production was still dominated by the supposedly "scientifically founded" theory that the breed qualities of seed cannot be improved. Johannsen's theory of pure lines, denial of heredity variation of organisms under the influence of their conditions of life, a mechanistic interpretation of the processes of fertilization, and denial of the constructive role of selection served as the "scientific" basis of seed production. Only after the work of Academician T. D. Ly-

senko did it become possible radically to reconstruct the work of seed growing. Since 1938 seed production in our country is based on the theory of plant training, intravarietal crossing, and selection. It is precisely on this basis that our plant-breeding stations began to produce élites with superior yields.

The All-Union Institute of Selection and Genetics and several other plant-breeding institutions are now engaged in elaborating a new method of increasing the yield qualities of seed—by intervarietal hybridization under conditions of open pollination. I shall dwell on the first results obtained by the Institute in this work a little later.

Under the influence of the Mendel-Morgan theory, the method of inbreeding (enforced self-pollination) was widely practised in the breeding of cross-pollinating plants. More, this method was regarded as fundamental in the breeding of cross-pollinators, as a consequence of which much time and means were wasted by breeders in vain. Not infrequently this method caused direct harm, especially in stockbreeding. The work of the Michurinists on the biology of fertilization made it possible to demonstrate the unsoundness of this method, which was a product of formal genetics.

The effectiveness of Michurin's teachings is clearly demonstrated in the matter of planning the plant-breeding process. Formal genetics denies the very possibility of planning the breeding of new forms. This explains why the Mendelist-Morganists attacked Academician T. D. Lysenko so fiercely when he began to demonstrate experimentally that by proper selection of the parental pairs, new varieties of agricultural plants can be developed by the method of hybridization in a short, planned period.

The production, on the basis of the theory of proper choice of the parental pairs for crossing, of the Lutescens 1163 and Odessa 13 varieties of spring wheat and the Odessa 14 variety of spring barley, as well as the production of the Odessa-1 cotton variety, which was obtained on the principle of roguing the first generation of hybrids, has provided brilliant corroboration of the possibility of planning the breeding process, if the work is governed by Michurinian theory.

Academician T. D. Lysenko has already in his address expounded the fundamentals of the Michurin theory of directed alteration of the heredity of organisms by means of training.

I shall give a few relevant examples from the work of our Institute.

By sowing Pallidum 32 spring barley in autumn, a winter barley was obtained which in respect to winter-hardiness is superior to any of the existing varieties of winter barley. Furthermore, this new variety matures much earlier than winter barley (which is of great importance for the South). For example, in 1948 the new variety of barley began to ear 7-8 days earlier and to mature 5 days earlier than the standard Krasny Dar winter barley.

The Institute is now testing varieties of spring wheat obtained from winter wheat by the method of directed alteration. These new forms of spring wheat approximate in yield the best of our standard spring wheats, while Novokrymka 204 even surpasses them. As regards the technology of production, the new varieties of wheat either closely approximate to the standard varieties, or are superior to them.

It is also interesting to note the behaviour of the new wheat varieties in respect to resistance to stinking smut. It is known that Hostianum 237, and especially Ukrainka, are non-resistant to stinking smut. But when they were converted into spring varieties by means of directed training, they acquired resistance to this disease. For instance, when they were sown under conditions which exposed them to the disease, that is, were artificially infected with the spores of the smut, winter Ukrainka and Hostianum 237 were infected to the extent of 95% and 89%, respectively; but these same varieties, when converted into spring forms, were infected 4% and 3%, respectively. Thus the winter varieties, which are non-resistant to stinking smut, become highly resistant when converted into spring varieties.

It was ascertained that directed alteration of the nature of plants by means of training results not only in altered requirements of the plants in the vernalization phase, but that also, in the case of some plants, other characters likewise change. For example, in the case of wheat, changes are observed in the pubescence of the cotyledons, colouring of the ears, character of the awns, length of vegetative period, etc.; in the case of barley, we observe a transition from multi-rowed to two-rowed (and vice versa), changes in awns, and the like.

Thus the converted spring varieties of wheat of which we

are speaking are not uniform as regards genotype. Some are better, some are worse. By means of selection from this material, strains have been obtained with definite practically valuable characteristics. For instance, one of the strains of Kooperatorka spring wheat is distinguished by its very early maturation; in the variety tests this year it eared two days sooner than the most early-maturing variety, Lutescens 1163. Other strains obtained from Kooperatorka and Ukrainka are distinguished by enhanced vitreousness of the grain, which approximates to that of hard wheat. Without predicting that new varieties may be obtained from this material in the near future, it may however be definitely said that it represents considerable interest for breeding purposes.

If to what has been said it be added that, as Academician T. D. Lysenko told us here, varieties of winter wheat with high winter hardiness have been obtained by the method of directed alteration, it will be obvious that directed alteration of the nature of plants is becoming an established method in plant-breeding work.

A striking example of the possibility of directing changes in the nature of plants on a mass scale is the summer planting of potatoes. As we know, the problem of cultivating potatoes in the South Ukraine was solved by the method of summer planting. Summer planting not only puts a stop to the degeneration of seed potatoes, but tends to improve their breed, as has been convincingly corroborated by experiments and in the course of extensive practical application on the collective farms.

Vegetative hybridization is also becoming one of the established methods of plant breeding. Vegetative hybrids obtained by grafting tomato on nightshade (*Solanum*) and *Lycium barbarum* are now undergoing fundamental varietal trials. It may already be said that, as far as early-maturation is concerned, these vegetative hybrids are not inferior, or are even superior, to the earliest maturing tomato varieties. As regards yield (as may be judged by the number of fruits), these hybrids are considerably superior to the early-maturing varieties. The vegetative hybrids are distinguished by one valuable quality—they show no tendency whatever to shed buds or ovaries, whereas shedding of buds and ovaries in one degree or another is characteristic of all known varieties of tomato.

Permit me to dwell in greater detail on the work done in connection with the new method suggested by Academician T. D. Lysenko of improving the breed qualities of the seed of old varieties and producing new varieties—a method based on hybridization by means of open pollination, which provides optimal opportunities for biological election at the moment of fertilization. The Institute is testing this method on winter and spring wheat, maize, rye, sunflower, barley and cotton. This set of cultures should ensure the solution of the problem in general, as well as of a number of individual problems connected with each culture in particular.

In this work we are guided by the following, already known, principles:

1. Crossing lends vigour, greater vitality, to the offspring.
2. Hybrids possess greater plasticity than the parent forms, which were propagated for long periods as pure strains.
3. In the process of fertilization, election is manifested and, consequently, if the characteristics of biological adaptation coincide with practically useful characteristics, this property, this regularity, may be used for breeding purposes. It follows from this that hybridization of the best varieties for the given locality, of winter wheat, for example, by means of open wind-pollination will enhance the vigour of the initial maternal varieties and their resistance to various adverse influences, and will hence increase their yield.

In this work we are also guided by the large amount of experience already gained in intravarietal crossing of self-pollinators and additional pollination of cross-pollinators. At the same time, a large amount of data has been accumulated in respect to each culture in particular. For example, in the case of winter wheat, D. A. Dolgushin's prewar experiments in this form of hybridization already yielded the first positive results. Similar work is known to have been done on rye (Avakian, Glushchenko and others), and it shows that when intervarietal pollination is employed, the type of the variety is fairly well preserved and its yield is increased. Intervarietal hybridization of maize is already being applied in practical farming (although, it is true, so far only in respect of the first generation). In the case of cotton, the large amount of hybrid material obtained by the Institute from the crossing of over

one thousand combinations of parental pairs indicates that the hybrids have a higher yield than the parental forms. Similar material has been obtained in the case of other cultures.

These, in brief, are the initial data for the tackling of this theme. The first results already obtained indicate that the Institute is on the right road.

In 1945, a large amount of hybrid material, 59,000 grains, was secured from the pollination of our best varieties of winter wheat, Odessa 3 and Odessa 12, as well as two other varieties which were grown (and are still being grown) on large areas—Ukrainka and Hostianum 237. These varieties were pollinated (by means of wind action) with the mixed pollen of many other varieties. We are now in a position to study the first, second and third generations of these hybrids.

As a result of detailed and all-round study, it is already possible to draw one definite conclusion: in the case of four varieties we have obtained seed with higher yields; the hybrids are more robust, better adapted and more fecund. And since these varieties are in themselves the best for many regions of the Ukraine, it may be confidently said that the Institute now possesses nearly a hundred centners[1] of winter-wheat seeds which are capable of giving the highest yields in the southern regions of the Ukraine.

I have a request to make to the Academy and to the Ministry of Agriculture, and that is to allow us to sow the hybrid varieties of Odessa 3 and Odessa 12 as super-élites, and to put them to extensive trials on the state variety testing plots in the southern regions of the U.S.S.R. As to Ukrainka and Hostianum 237, it would be better to put them to broad trials on the variety testing plots in regions where these varieties are accepted local standards.

In the case of maize, the work is being conducted with a view to utilizing for practical production, not only the first but also the second, third and subsequent hybrid generations. It is believed that if certain requirements are observed it may be possible to obtain high-yield hybrids both in the first and following generations. The work of the Institute shows that these requirements are as follows:

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[1] The centners in this book are metric centners=100 kg.

1. The maternal plants chosen for crossing must be taken from the variety with the highest yield in the given district, and the paternal pollinators selected from other good varieties. The initial seed for the crossing must be taken from plots with high yields, and where additional pollination was effected. As an example showing that cross-pollination may enhance the yield qualities of the seed, I shall give data relating to tests of ordinary and cross-pollinated élites. In the case of the Brown County variety, the yield from cross-pollinated élites in 1939 was 19.5 centners per hectare, whereas that from the ordinary variety was 16.2 c. per ha.; the respective figures for 1940 were 32.7 and 28.5 c. per ha., and those for 1947, 39.9 and 34 c. per ha. Incidence of infection with the smut *Ustalago maydis* in 1939 was 3.4% for the cross-pollinated élites and 9.4% for the ordinary; in 1940—4.4% and 8.2%, respectively. In the case of the Grushevskaya variety, the yield per hectare in 1947 was: cross-pollinated élites—40.4 c., ordinary—36.2 c. As we see, cross-pollination improves the yield quality of the initial material. The varieties taken for our experiments in hybridization of maize were Brown County and Minnesota, and Brown County and Grushevskaya. The variety with the higher yield—Brown County—was taken as the maternal plant. The importance of the maternal variety in hybridization is shown by the following data. In the 1947 tests, the yield per hectare from the Brown County × Minnesota 23 cross was 36.5 c., and that from the reciprocal cross (Minnesota 23 × Brown County) was 29.3 c., or 7.2 c. less.

2. The varieties are planted for hybridization in alternating rows of the maternal variety and the paternal variety. During the flowering period repeated additional pollination is practised. Hybrids obtained in this way prove to be of higher yield and more resistant to smut than hybrids obtained by the method of planting usually practised (one row of the paternal variety alternating with two rows of the maternal variety, and without additional pollination). For example, in last year's tests hybrids obtained by the method practised at the Institute yielded a harvest of 41.6 c. per ha., while the ordinary hybrids yielded 36.1 c. per ha. Incidence of infection with *Ustalago maydis* when induced artificially, was 32% and 48%, respectively.

3. The hybrids must be cultivated under the best agro-

technical conditions, and systematic cross-pollination and selection must be practised.

I repeat that there are grounds for believing that if Brown County and Minnesota 23 and Brown County and Grushevskaya are taken for crossing, if the seeds are chosen from plots with high yields and where cross-pollination has been effected, if the seeds are planted with a maternal row alternating with a paternal row, if, further, during the period of flowering additional cross-pollination is effected several times, and if the hybrids thus obtained are grown under good agrotechnical conditions, the plants additionally pollinated several times, and the best cobs are selected for seed—then the yield of the second generation will not be less than that of the first.

Similar work done on sowings of the second generation will result in high yields in the third generation, and so on.

This year the Institute is studying first, second and third generations of hybrids. It is still too early to speak definitely of the results of the trials. But, to judge by the present development of the plants of the second and third generations, they are to all appearances not inferior to those of the first generation. A count of cobs formed as of July 24 showed that on the average their number per plant in the first, second and third generations is approximately equal. If anything, the number of cobs formed on the plants of the second and third generations is even greater. The cobs per plant in the first generation average 2.11, those in the second—2.33, those in the third—2.48. There is therefore reason to assume that, as far as yield is concerned, the material of the second and third generations is at any rate not inferior to that of the first.

As regards rye, in order to obtain higher-yield seed, last year four of the best varieties for our district were cross-pollinated, viz.: Petkus Veselopodolyansky, Petkus Kharkovsky, Tarashchanskaya type 2 and Tarashchanskaya type 4. All the cross-pollinated strains have this year been put to variety trial. We believe that the cross-pollinated varieties will give a higher yield than the initial varieties. This year we have already gathered about 20 centners of seed of hybrids of Petkus Veselopodolyansky, which is a standard variety for our district.

In the case of sunflower, we are now studying the second generation of hybrids, obtained by open cross-pollination in a

special sowing of six of the best varieties—Zhdanovsky 8281, 1813, 1646, 4418, 3519 and 4036. We know from our prewar experience that hybridization enhances the yield of sunflower. But we still do not possess enough data as to the influence of hybridization on such important characters of sunflower as the oil and husk coefficients.

We have certain data on this question only in relation to additional pollination. For instance, the husk coefficient of the seeds of Zhdanovsky 8281 obtained from additional pollination was 39%, and that of the ordinary seeds—42.3%; oil content in the kernels was 51.8% and 49.0%, respectively. As we see, additional pollination improves the quality of the seed in respect to oil and husk coefficients. We now have initial data which indicate that, after intervarietal hybridization of suitably chosen forms, the oil content is likewise increased and the husk coefficient decreased. For example, an analysis for oil content of the first generation of an intervarietal hybrid of Zhdanovsky 8281 showed the following results: standard Zhdanovsky 8281 had 54% oil in the kernel, but all the strains of this variety obtained from pollination by other varieties had a higher oil content: 35 strains showed up to 59% oil, 19 strains—from 59% to 61.5% and 6 strains—an even higher ratio (up to 64.6%).

Husk coefficient: the husk coefficient of the standard Zhdanovsky 8281 used in this experiment was 41.3%, but the coefficients of all the strains of this variety obtained from intervarietal hybridization were smaller: 5 strains were near to the standard; 42 strains—36%-40%; 13 strains were considerably lower—32%-36%.

To judge by the seedlings and the subsequent development of the plants, this year's experiments likewise confirm that the hybrids have greater yield qualities. Any person, even if he was not an expert, who walked through the variety trial grounds, could unmistakably tell, judging solely by the external appearance of the plants, on which plot the élites of a given variety, and on which its hybrids were sown; the hybrids of all the varieties were sturdier, taller, and of a deeper green colour. Measurements showed that the heads of the hybrids were also somewhat larger than those of the control plants. For instance, in the case of Zhdanovsky 8281, the heads of the élites measured 21.7 cm. in diameter, and those of the hybrids (the

second generation in this case)—22.6 cm.; the measurements of variety 1813 were 21.7 cm. and 21.9 cm., respectively; those of variety 1646—21.5 cm. and 22.7 cm.; those of variety 4418—22.6 cm. and 23.5 cm.; those of variety 3519—20.4 cm. and 23.1 cm.; those of variety 4036—22.3 cm. and 24.1 cm.

If it is confirmed this year that the second-generation hybrids have a higher yield, higher oil content and smaller husk coefficient than the controls of the same varieties, we may then consider producing, beginning with next year, élites of Zhdanovsky 8281 by pollinating it with a mixture of the above-mentioned varieties. This year we shall have about 20 centners of seed of Zhdanovsky 8281 hybrids. This may prove to be the most fecund and highest oil-content sunflower seed for the southern regions of the Ukraine.

Interesting results were obtained in the variety tests indicating that barley hybrids have been obtained which possess a greater plasticity than our best varieties, Odessa 9 and Odessa 14. In 1946, which was a dry year, the later-maturing Odessa 9 gave a smaller yield than early-maturing Odessa 14; but in 1947, which was distinguished by drought in the first half of the vegetative period and by rains in the second, Odessa 9 gave a higher yield than Odessa 14. But how did the hybrid populations behave in these unfavourable years? In the dry year 1946, the hybrids (Odessa 14×Odessa 14)×Umansky, (Krimsky 17×Odessa 18)×(Medicum 81/7×Odessa 11), and Odessa 9×Umansky gave a yield higher than, or approximating to, that of early-maturing Odessa 14, and in 1947 higher than, or approximating to, that of Odessa 9, which gave a better crop that year. Averaging the two years, the yield of the hybrids was higher than that of either of these varieties. It will thus be seen that the hybrid plants, being more plastic, suffered less from the drought of 1946 and made better use of the favourable conditions of the second half of the vegetative period in 1947.

In trials of 20 hybrid populations of barley, the most fecund proved to be chiefly combinations obtained by crossing first-generation hybrids with a third variety or with the first generation of another combination of crossings. For instance, in 1947, the combination Medicum 81/7×Ganna Loosdorfskaya (the sixth generation was tested) gave a yield of 19.8 c. per ha.; the yield of a cross of a first generation of this combination with

Odessa 18 (sixth generation) was higher, 21.1 c. per ha.; the yield of a cross of this same first generation with the first generation of the combination Krimsky 17×Odessa 18 (sixth generation) was still higher, 22.6 c. per ha.

Last year, selection nurseries of the best hybrid populations of the sixth and seventh generations were laid out; the seed of plants selected from each population have been sown for further assessment and selection. Seeds of the best strains from each of the selected population, several hundred in number, will be mixed and sent for testing and multiplication as new varieties.

By means of culling of first-generation cotton hybrids, an early-maturing, high-yield variety was obtained: Odessa 1. During the work on this variety, a large number (over 1,000) hybrid combinations were studied with the object of ascertaining the manner of development of the first and second generations. It was found that when our early-maturing varieties are crossed with many other forms, we get hybrids which are conspicuously superior to their parents in sturdiness of the plants, early-maturation, size of bolls, quantity and quality of fibre and, as a result, in yield. When the crossed varieties were of the best for our district, the hybrids in the second and following generations behaved better than their parents.

In 1946, three of the best first-generation combinations were selected. Having grown the second generation in the winter 1946/47 in greenhouses, we obtained the fourth generation in the winter of 1947/48, and this year we are already in a position to plant out in the field the first, second, third, fourth and fifth generations of three selected combinations, for observation under comparable conditions. Comparing variations of the major agronomic characters—yield, early maturation, size of boll, leaf spot resistance, and quantity and length of fibre—in the five generations, we may arrive at authentic conclusions as to the use of these hybrid populations as new varieties. Inasmuch as the preliminary study made of three generations last year, as well as the afore-mentioned observations and theoretical considerations, point to the possibility of producing varieties in this way, the cotton-breeding department of the Institute has undertaken to produce a new early-maturing and high-yield variety of cotton this year, and to obtain 100 kg. of seed for its further multiplication and study.

To sum up, it may be said that a sufficiently large amount of experimental material is available to make it possible already this year to form a reliable judgment of the new method of obtaining high-yield seed.

A few words on the subject of regulating hybrid segregation. The formal geneticists deny the very idea of this. Ten years ago, when I had occasion to have the plan of research of our Institute endorsed by a commission of the Lenin Academy of Agricultural Sciences, which included the theme "Regulation of Hybrid Segregation," one prominent geneticist demonstratively left the room, declaring that he could not even remain present at a discussion of this problem. The Institute has now obtained the first experimental data which convincingly prove the possibility of regulating hybrid segregation.

The segregation of hybrids may be regulated by placing them under suitable conditions of training. Second-generation hybrids of several spring varieties and winter varieties, when planted simultaneously in the field in spring, displayed different spring-winter proportions, depending on the conditions of growth of the first generation. The proportion of spring forms in the second generation was least in those cases when the first generation had been sown in the field in autumn. The proportion of spring forms was conspicuously greater in those cases when the first generation had been sown in the field in spring, and greatest of all in those cases when the first generation had been grown under glass.

Among the second generation absolutely non-segregating families were discovered. A study of the third generation confirmed the constancy, stability, of many of these hybrid families.

As you see, the assertion of formal genetics that the segregation of hybrids cannot be regulated has also proved to be fallacious. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician J. H. Eichfeld.

Academician J. H. Eichfeld. The resumption in late years of the acute discussion on biological issues, notably on the heredity and intraspecific relations of plants and animals, points in the first place to the widened outlook of our scientific

forces, especially of our agronomical scientists, who find they cannot cope with their tasks without a correct biological theory.

On the other hand, the discussion is indicative of the existence of serious divergencies among the biologists of our country, of their division into two, sharply differentiated groups. The first group comprises the supporters of the Mendel-Morgan genetics, who often, without any justification whatsoever, call themselves orthodox Darwinists.

The second group consists of the supporters of the Michurin theory, headed by Academician T. D. Lysenko. This group is creatively developing the teachings of Darwin and Lamarck, boldly discarding, as was quite distinctly pointed out in the address of T. D. Lysenko, individual erroneous views and interpretations characteristic of these two great scientists.

At the same time, we are entitled to note the following characteristic fact. For the first trend, if we may put it so, are working numerous institutes and laboratories of the Academy of Sciences of the U.S.S.R.; they are, moreover, engaged exclusively in this work, and have no direct connection with production. This puts them in a privileged position; they are working for themselves, as "private gentlemen," to use Timiryazev's figurative expression. Furthermore, they have at their disposal the overwhelming majority of the chairs in the biological faculties of the universities, and in the agricultural, pedagogical and medical colleges.

The adherents of the Michurin trend are in a less favourable position. Of the institutes of the Academy of Sciences of the U.S.S.R., only one, the Institute of Genetics, is following the Michurin path. True, there are numerous agricultural research institutions in the Soviet Union which also work in the field of biology, but it should not be forgotten that they are overladen with the purely practical problems assigned to them and, unfortunately, do very little work in the realm of theory.

Nevertheless, as we see, the Michurin trend cannot be, I would say, pushed out of the picture (and still less suppressed) by the opposite side, as has often been predicted in whispered conversations. On the contrary, this trend is acquiring more and more supporters, especially among workers at plant-breeding stations, in agricultural research institutions, and among the foremost people on the farms—that is, among people who

are directly engaged in practical problems, and who are most of all interested in having a correct theory to help them successfully cope with the problems they are called upon to tackle.

This goes to show that the Michurin trend in biology is a correct and effectual trend, otherwise it would not attract the forces who are directly interested in the progress of our agriculture. It helps our practical agriculturists to cope with complex and honourable production tasks. The Michurin trend in science illuminates the path of production and assists its advancement and success. That is why we find so many supporters of this trend among practical workers in agriculture. The strong point about the Michurin trend is precisely that it helps production. At the same time, it helps agricultural scientists to withstand the onslaught of the exponents of backward trends in biology, it promotes the development of progressive biological science.

Some time ago, last winter, Academician Pavlovsky delivered a lecture in Leningrad on "The Soviet Stage of Darwinism" under the auspices of the Society for the Propagation of Scientific and Political Knowledge. He stressed the point that in our time, as in the time of Darwin, fundamental biological problems are solved at agricultural enterprises and by people directly connected with agriculture. That is perfectly true.

As we know, vitality is the distinguishing feature of everything progressive; and it is to the progressive that the future belongs. This is exemplified in our day by the development of the Michurin theory. That in the first place.

In the second place, the achievements of the Michurin trend in biology are due to the fact that it has assimilated all the best, everything that is progressive handed down to us by preceding generations of Russian and Soviet biologists. T. D. Lysenko spoke of this in his address. We may justly be proud that in pre-revolutionary Russia and in the Soviet Union lived and worked such distinguished biologists as the Kovalevskys, Sechenov, Mechnikov, Timiryazev and Michurin, who have developed biological science on Darwinian lines. In the eighteenth and nineteenth centuries, there were no Weismanns, Batesons, Lotsys or Morgans in our country. All the more regrettable is it that nowadays many of our biologists laud and extol, not pro-

gressive scientists, our fellow-countrymen, but representatives of reactionary, idealistic trends in science abroad.

More than eighty years ago, as T. D. Lysenko pointed out, I. M. Sechenov taught us to regard living beings in their interconnections with their entire surrounding environment, animate and inanimate. The views expressed by T. D. Lysenko fully coincide with the views of I. M. Sechenov. They have given a correct biological direction to our thought, they have induced us to imbue our entire agricultural science with biological conceptions. Biological conceptions are now penetrating into all branches of agriculture, from breeding, seed growing and agrotechnique to mechanization and other branches.

This profound revolution in the approach to agricultural problems cannot be denied even by the opponents of the Michurin trend, much as they would like to. We have learned to examine the processes of agricultural production with the keen and appraising eye of the biologist. This has given, and is giving, us many advantages in tackling the most complex problems of plant growing and stockbreeding.

The truth of this may be verified by turning to the practical work of the majority of our agricultural research institutions, especially the plant-breeding stations. A no less fruitful influence has been exercised by the agrobiological trend on the big, central institutes.

Let us take as an example the All-Union Institute of Plant Industry. There was a time when attempts were made to turn this Institute into a stronghold of the Morgan-Bateson genetics. However, under the beneficent influence of agrobiological science, the highly qualified, industrious, sincere and earnest body of workers of the Institute succeeded in repelling these attempts. They are steadily and persistently converting the Institute into an advanced scientific research institution. This enabled them during the war to engage, not in the study of flies but in serious work aimed at enlarging the food resources of the industrial areas of the Urals. The work begun in the Urals during the war on forage grass, vegetable and potato seed growing, as well as on weed control, is entirely based on Michurin's teachings. This work is now also making it possible to advance the theoretical principles governing the production of stable varieties of forage grasses. Unfortunately, I cannot dwell on the

details, but I have frequently dealt with these questions at meetings of the Academy.

It is to these teachings, too, that the Institute owes the progress it is making in solving two other many-sided problems: the geographical advancement of agriculture into the Far North, and into the desert areas of the U.S.S.R. The early maturing varieties produced by the Hibiny Station, which are helping to promote agriculture in areas of the Far North where we formerly never believed farming could be pursued with success, were bred and improved on the basis of the Michurin theory. The Michurin theory likewise threw light on many new facts relating to the behaviour of plants and having an important bearing on theory and practice, which had been accumulating at the Hibiny Station in the course of years and years of work, and which could not be theoretically explained from the standpoint of Mendelism-Morganism.

Research into problems of the dormancy phase raised by Academician T. D. Lysenko has also made it possible to produce at Hibiny ultra-early maturing varieties of potatoes which, when grown in the South, yield two harvests in one summer.

The majority of the varieties bred at Hibiny were produced on the basis of a preliminary analysis of the initial material from the standpoint of the theory of phasic development.

We are convinced that, in the light of the Michurin theory, collections of the world's cultivatable plants acquire immeasurably enhanced importance.

When studied in diverse natural conditions, this rich material, sometimes even independently of the efforts of the researcher, reveals remarkable laws of plant development, reveals new ways and causes of variation. New facts not observed before, when regarded from the point of view of Michurin's teachings, throw light on the origin and evolution of this or that form, and on how to utilize it properly.

The practical value of this new approach to the study of world collections is borne out by the incontestable fact that, in the difficult and trying conditions of the war and the post-war period, the Institute alone produced more than 170 new varieties, and that in 1949 their number will be increased to 200.

From this new standpoint, too, the Institute is tackling problems relating to the systematics of cultivated plants, and

it must be said that the Michurin theory is of no little help in this work as well.

The laboratories of the Institute at Pushkin were only very recently lying in ruins—there were no greenhouses nor hothouses. Everything had been wrecked by the Germans. Now the laboratories have certain facilities for work.

The postwar researches carried out on the basis of the theory of phasic development confirm the correctness of Michurin's views and controvert the theory of foreign and some of our Soviet investigators that the processes of vernalization and hardening of organisms are reversible; they throw light on the connection between these processes and heredity.

Advances are also being made in the theory of directed variation of heredity. The biochemical laboratory, which is just now only one-third of its prewar size, is working successfully to establish the dependence between conditions of growth and the process of accumulation of substances useful for agriculture. The conclusions from these researches should lead to substantial alterations in the system of field manuring.

Work on the resistance of plants to disease and pests, which is being conducted from the standpoint of the theory of phasic development of plants, also fully confirms this theory.

It has been established that the resistance of plants to pests and diseases depends less on the definite botanical variety to which the given form belongs, as was formerly believed, than on the conditions in which the process of development of the given form took place. It has been ascertained that resistance to parasites is not an unvarying character mechanically transmitted by heredity, but a biological property which changes and develops, dependent on the conditions in which the given organism developed.

The Institute possesses a vast amount of data which indicates that a variety which is non-resistant in certain conditions, when transplanted to other conditions and then, after several years of cultivation in the new conditions, returned to the former conditions, possesses entirely different characteristics from those it possessed before. This opens new approaches to the problem of breeding for resistance.

In the course of development of biological science in our country before the war, the Institute of Plant Industry was

severely criticized on account of its theoretical conceptions. The Institute is now undergoing a rebirth, under the influence of the Michurinian theory.

Its workers are making no little effort to shake off the influence of the old idealistic theories which were forced upon them, and to sharply face around to the progressive, Michurin teachings, to master them and to take an active part in their further development.

The Institute's workers realize that they have not yet by far given to agriculture all that they can give and should give. They are reconstructing their theoretical base all too slowly, and are all too slowly restoring their production base wrecked by the Germans.

All this is true, and the reproaches levelled at the Institute are deserved. But at the same time it should be pointed out that in recent years more abuse has been hurled by reactionary foreign "scientists" at the Institute of Plant Industry than at any other of our research institutions. We have asked ourselves why it is these scientists have conceived such a detestation for the Institute, which they formerly lauded and praised.

Evidently, the reason is that the Institute has altered its trend, changed its theoretical outlook. We are not disturbed by this attitude of reactionary scientists towards us; on the contrary, it gladdens us. If we are abused from that quarter, we must obviously be on the right path.

Unfortunately, a substantial group of Soviet scientists have of late years lent an attentive ear to the voice of these foreign "friends" and re-echoed it. And this has very seriously hampered our efforts both as regards theoretical readjustment, and as regards the rebuilding of our production base. It is to be hoped that we have already passed through this unpleasant phase, and that the Institute will be able in future more successfully to develop its research work for the benefit of our agriculture.

I shall take the liberty of dwelling on certain of our divergencies. T. D. Lysenko said in his address that the supporters of the Morganian genetics have not the courage frankly to admit that they are followers of foreign reactionary genetics. They pose as Darwinists, and some of them even call themselves orthodox Darwinists.

It is incomprehensible how people who profess autogenetic concepts in biology and who deny the influence of external conditions on the variability of organisms and on its direction, can call themselves continuers of the work of that eminent Darwinist, A. N. Severtsov, who, by his classic researches, established that phylogenetic variations were solely due to changes in environment, and that it is precisely these changes which determine the evolutionary process. A. N. Severtsov used to say that unless we accepted this thesis we could not understand the phenomena of adaptation.

A. N. Severtsov categorically denied the existence of some internal principle of development, residing within the developing organisms themselves and not dependent on changes of external environment.

A. N. Severtsov recognized the fitness of adaptive changes —and thereby in fact gave the answer to the question why forms undergo variation, why they evolve.

Agrobiologists who hold these, in our opinion, true views are dubbed by the autogeneticists Lamarckians, mechano-Lamarckians, and so on. The formal geneticists call the supporters of the Michurinian trend, not followers of A. N. Severtsov but Lamarckians, and themselves pose as continuers of his theory. Actually, however, theyare would-be destroyers of his theory.

The ideas and experimental work of the agrobiologists coincide in many respects with the teachings of A. N. Severtsov. It was he who taught us that it is absolutely necessary for an understanding of the theory of evolution as a whole to study the influence of separate groups of factors in the changing conditions of the external environment on heredity and its mechanism. This is precisely what the Michurinists do. This is also what Academician M. A. Olshansky told us today. What right have the autogeneticists to clap the sneering label of Lamarckian or mechano-Lamarckian on others?

The autogeneticists do this because they assume that the general run of Soviet people are unfamiliar with the work of the eminent Russian Darwinists and with the real substance of Lamarck's theory, and with the fact that the agrobiologists accept the good sides of his theory and reject the bad sides.

Lamarck has been made a sort of bugbear. It would therefore not be amiss to recall the words of K. A. Timiryazev, who

said: "With regard to plants, Lamarck stood on the strictly scientific basis of facts, and the views he expressed have retained all their significance today. He believed that variations in plants were exclusively due to the influence of external conditions—of environment."[1]

A. N. Severtsov, as you have already heard, affirmed the same view in respect to animals.

It is true that later Lamarck went back on his correct conclusions regarding the causes of variation in organisms. Timiryazev says: "Starting with the perfectly correct view that *external conditions influence the forms and structures of animals,* he then, pulling himself up, as it were, repudiated it in the following words: 'Of course, if anyone were to take these words literally he would be fathering an obvious mistake on me, since no conditions produce *direct and immediate modifications* in the form or structure of animals'."[2]

However, these temporary vacillations of Lamarck do not in our eyes detract from the significance of this great scientist.

To the end of his life A. N. Severtsov remained faithful to his conclusion that the factors of the external environment have a direct and fitting influence on variation. Soviet agrobiologists are developing these conclusions and are not ashamed of being consistent followers of A. N. Severtsov.

Lamarckism should not be used as a bogey today: this is unwise, to say the least of it. Anyone in the least acquainted with the history of biological science can be no more frightened with this word than with the word "chimneysweep," which was used to frighten little children. Soviet scientists have already emerged from the age of childhood, and they are not to be frightened so easily. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician I. V. Yakushkin.

Academician I. V. Yakushkin. Comrades, in the latter years of the tsarist regime, just before the October Revolution, it was often said that agricultural science had reached a dead

[1] К. А. Тимирязев, *Сочинения,* том VI, стр. 248.
[2] *Ibid.,* p. 76.

end. In relation to the impoverished and debt-laden small peasant farms of that period this was correct.

From the address of Academician Lysenko we have heard at this session, I think we are absolutely warranted in drawing another conclusion, namely, that we are now living in the great Stalin epoch, in the period of the triumph of the advanced agricultural science which has been built up on the celebrated work of Michurin and Williams, and which assists us in our struggle for a new world.

The results of the discoveries of Michurin and the Michurinists are well known to you all. The immediate disciples of Ivan Vladimirovich Michurin who are present here could very likely tell much better than I of the miraculous transformations of the nature of fruit trees achieved both by Michurin himself and by the Michurinists. We have hybrids of apple and pear, cherry and apricot, almond and peach, and many other new breeds of fruit trees. We have a Soviet gooseberry which in size of fruit is comparable to the grape, and varieties of currant whose berries are not smaller than gooseberries. Last year I had occasion to examine the fruit nurseries of the Latvian S.S.R. in the condition in which they were inherited from the bourgeois Latvian Republic—there the currants were like beads, whereas, I repeat, the Michurin varieties of currant yield fruit equal in size to the fruit of the gooseberry. Selective breeding and the application of the methods mentioned here of producing varieties by means of vegetative hybridization are the chief explanation of these remarkable results.

As Academician Olshansky has so vividly described today, in the hands of Academician Lysenko the Michurin methods have also yielded magnificent results, when applied to annual vegetable and field crops. T. D. Lysenko's discoveries are the theme of poets and writers, playwrights and philosophers. I cannot however refrain from saying here that in their descriptions they sometimes display an obvious unfamiliarity with their subject. The actual discoveries of T. D. Lysenko and his school, in my opinion, considerably surpass these descriptions.

I shall now give an illustration of the principle which, as you heard in the address of T. D. Lysenko, should be recognized as basic in seed growing. It is the principle that socialist agri-

culture must be based on a combination of Michurin's theory and Williams' theory. This principle also shows that the best seed qualities are possessed by plants taken from fields and plots which give the biggest yield. As we know, maximum yields are obtained most successfully and constantly from lands where the *travopolye* system of farming is applied in full.[1] The Department of Plant Industry of the Timiryazev Academy has been working for several years now on the application and development of this principle along the lines I have just mentioned.

Numerous experiments carried out by my pupils and collaborators have shown the possibility of achieving considerable improvement of the quality of seed material within the same variety by using seeds from plots with highest yields.

One effective method in this connection practised by us is timely sprinkling. Ever since the law on the irrigation of the Central Russian uplands was passed, this method may be considered as available for mass utilization on the kolkhozes.

I shall exhibit specimens of millet just brought from the Tambov Region. On the right you have millet which was not watered; on the left, millet which received one watering, at the end of May. The vegetative mass of the second specimen is five times as great as that of the first, and it cannot be doubted that the quality of the seed taken from the irrigated plots will be incomparably superior to that from the unirrigated plots.

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1 By *travopolye* is meant a system of agronomical measures founded on the teachings of the eminent Russian scientists V. V. Dokuchayev (1845-1903), P. A. Kostychev (1846-1895) and V. R. Williams (1863-1939).

The travopolye, or Dokuchayev-Kostychev-Williams, system ensures the rational organization of the entire agricultural area (fields, woods, meadows). It includes the afforestation of watersheds and the planting of shelter belts and windbreaks, as well as the introduction of two interconnected rotations: field crops and forage grasses (a mixture of perennial grass seed is indispensable). Methods of cultivation and soil manuring are likewise defined. The travopolye system ensures a proper and harmonious relation between arable, stock raising and other branches of farming.

The Plan of Shelter-Belt Planting, Introduction of Lea Rotations and Construction of Ponds and Reservoirs adopted by the Council of Ministers of the U.S.S.R. in October 1948 provides for the application of the Dokuchayev-Kostychev-Williams system of agriculture on an extensive scale in the Soviet Union.—*Ed.*

This principle is, of course, still more widely applicable in the case of seed plots located on land where the travopolye system of farming has been developed to the full. Here, in this second exhibit, you may see the effect of shelter belts, as produced in 1947 in the Kuban, on the Stalin State Farm. The winter wheat yield in the open field was 10 c. per ha. (1947 was no less a dry year in the Kuban than 1946). The yield from sections protected by shelter belts was 27 c. per ha., or nearly three times as large.

The quality of the seed taken from fields sheltered by belts was incomparably superior to that of the seed from the open fields. The absolute weight of 1,000 grains of wheat was 25.5 g. in the open steppe, and 34 g. in the sheltered fields. When judging the comparative value of seed material, we consider even a minimal difference in the weight of 1,000 grains, a difference of 3-4 g., of substantial importance. Hence the proposal which I published in the spring of 1948, but to which our agricultural authorities have not paid sufficient heed, namely, that in all areas where there are shelter belts, even if they are not very wide, land located near to the belts should be selected for the seed plots.

Among the new attempts to improve the quality of seed material, mention should be made of spraying of cereal crops at the moment of emergence of the ears—and not with any special substances, but only with phosphates or potassium phosphates. These attempts are based on the special role which, as modern biochemistry has established, is played by salts of phosphoric acid.

I request you to examine the two test tubes which I shall hand round. A comparison of them will help you to get an idea of the difference we have achieved.

The usefulness of this experiment occurred to me in connection with the splendid results obtained in 1947 by the wide use of spraying from aeroplanes as a means of feeding winter crops. This method may also be used for the spraying of sugar beet just before harvesting, which considerably enhances its sugar content.

As the 1948 experiment showed, this method may also be employed with success, at any rate on the seed plots of the state seed farms, after the grain has eared, when no other means of applying nutriment is possible.

Such spraying, our experiment revealed, is accompanied by increased yield of crop and, most important, by enhanced quality of grain.

I think that anyone who knows how to determine the quality of wheats will not fail to admit that a distinct difference is to be observed here in the size of the grain, not only in the first test tubes, which belong to the second exhibit, but also in the second test tubes.

Thus it is a fundamental principle of agronomical science that the best varieties must be combined with high yields, which are obtained by improving agricultural methods.

Academician T. D. Lysenko has said that even the best varieties may easily be spoiled by bad agricultural methods, and that a high level of agricultural efficiency is necessary to maintain the quality of varieties.

Turning again to the general question, I should like to make a few remarks which may perhaps not be without use in the general consideration of the differences between the supporters of Michurin genetics and the formal genetics, which in my opinion it would be more correct to call reactionary genetics.

It would be wrong to say that the underestimation of Michurin genetics is not diminishing. According to my observations, this underestimation is on the wane both in universities and research institutions. But it is still very considerable. Yet only Michurin genetics opens up brilliant prospects for achievement in agricultural science. Not every one yet believes this, but I think the number of such people is decreasing from year to year, from month to month, and from day to day. Of these people one might say in the words of Engels in *Anti-Dühring*: in looking at things at rest they leave their motion out of account, because they cannot see the wood for the trees.

The President, in his address, and today's speakers have given concrete examples illustrating the splendid results obtained by following the Michurin way.

I think it would be more correct to connect the theory of heredity, as interpreted in the works of Michurin and Lysenko, with physiological theory. This would be fully in accordance with the views of Timiryazev. The starting point here is metab-

olism; hence, the extension of the influence of the uprearing of organisms to their physiology is the decisive factor in producing changes of heredity.

I cannot help remarking, and those present here will agree with me, that the researches of one of our Morganist-Mendelists on the influence of the Great Patriotic War on the chromosome structure of flies, referred to by T. D. Lysenko in his address, are the utter limit.

At a time when all true sons of the Soviet people were fighting for the honour, independence and liberty of our country and carrying that fight to a victorious conclusion, there were researchers who devoted their time to the study of the influence of the war on flies!

A voice. Flybreeders!

I. V. Yakushkin. Such extremes, it seems to me, only show how wrong it is for any Soviet scientist to isolate himself from the life of his country.

It is sometimes said that the Michurinists and Morganist-Mendelists are united by the fact that they both widely apply the method of hybridization. But the ways they understand hybridization widely differ. The Michurin geneticists attach great importance to intravarietal and intervarietal crossing. Intravarietal crossing is rather in the background at present, but we know that it yielded very good results even when applied to varieties which at that time were called pure lines.

The effects obtained by Academician T. D. Lysenko with the help of intervarietal crossing of rye have already been referred to. They are much more significant than is usually thought or said. Rye élites produced in this way proved in the variety trials to be much more prolific and less susceptible to a number of funguous diseases.

It is sometimes said that T. D. Lysenko's adverse attitude to inbreeding has retarded the creation of stocks of hybrid maize seed. In actual fact, the application of this method and the accumulation of hybrid seed was retarded, not by the Michurin school but by those breeders who were eager to imitate American practice and insisted upon prolonged preliminary inbreeding.

I have already had occasion to say, and I am glad of the

opportunity to say it again at this session of the Academy, that many of our plant breeders do not understand the practices of American capitalist seed firms. Way back in 1929, we tested at the Voronezh Institute intervarietal crossing of different types of maize, and arrived at the conviction that a big increase of yield may be obtained by such crossing even without preliminary inbreeding, and moreover, with considerable difference of varieties.

In regard to other, non-cereal cultures, the work of T. D. Lysenko has disclosed a number of new laws. Under this head may be mentioned the new ways of topping cotton plants, which is a unique method of pruning the plants, and the propagation of kok-saghyz from cuttings—a method which within two or three years will cause a revolution in the cultivation of rubber-bearing plants. Summer planting of potatoes marked an epoch in potato growing, since this method improves the sowing material not only in the southern areas of the country, but also in the central belt. Many of you will recall the facts published by T. D. Lysenko in his book *Agrobiology*, from which it is seen that repeated summer planting may increase the average weight of the tubers several times over, from 200-300 g. to 1 kg. and more per plant.

The principle of repeating the action (upon a plant) of the conditions to which the breeder or agronomist is endeavouring to adapt the plant organism, and in which he intends to cultivate it, has made it possible to transform wheat (to change its heredity). This transformation of wheat was achieved by bringing influences to bear on the plants at crucial moments of their development. Those who went with the excursion to Gorki-Leninskiye yesterday will recall some small, but very remarkable, plots. There we saw that typical spring wheat No. 321, when planted in autumn for four generations in succession, is converted into winter wheat. Planted in spring, this wheat no longer produces spike-bearing stems. The converted wheat obtained from the spring variety surpasses in winter hardiness all the most winter-hardy forms.

This conversion of winter wheat into spring wheat, and vice versa, is not of theoretical significance only. We all here know very well that winter varieties, if they have overwintered well, give much higher yields than spring varieties. When a spring

wheat is converted into a winter wheat, its requirements with regard to temperature regime undergo a radical change. This opens up the possibility of creating frost-resisting varieties of cotton, or of converting biannual forms of aromatic and oil plants (thyme, etc.) into annual or spring forms.

You saw the wonderful fields of ripening branched wheat. They are as much a discovery for the Moscow area as the cultivation of grapes in the vicinity of Michurinsk, and a no less important one. For, as you know, this kind of wheat was grown for many years in nurseries, but always with unfavourable results and the quality of the grain was extremely poor; it was a southern wheat and was considered unsuitable for the central belt. T. D. Lysenko succeeded in finding a method of cultivation which made this wheat promising for many areas in which it is entirely new. We know that this wheat produces 200 grains where ordinary wheat will produce only 30 or 40. Where a spike of ordinary wheat will yield 1-1.2 g. of grain, a spike of branched wheat may yield not only 3 g., as we cautiously say, but 4-5 g. In other words, in fecundity, a spike of this wheat is equivalent to a tuft of millet. One highly important feature of branched wheat, of course, is that it is absolutely free of the tendency to lodge. It is saved from lodging, not only because of the thickness of its stem but also because of the thinness of stand. One million five hundred thousand ears of branched wheat will give a yield equivalent to that of five million ears of ordinary wheat. Here, consequently, the task is the reverse of the one formulated by Timiryazev, when he spoke of growing two ears where one was grown before. Here it may be more expedient to grow one ear, but a highly productive one, instead of three ears.

In some of the spikes which T. D. Lysenko kindly handed me, we were able to count 120-125 spikelets. And in view of the large number of grains in each spikelet, here we may expect to get 300-350 grains per spike.

I should like in conclusion to express the conviction that the development of the principles of Williams, Michurin and Lysenko will enable agricultural science to multiply its achievements, on new and immense scales, and with greater rapidity, in order that agricultural science, as part of an integral Soviet science, may help to speed the coming of Communism,

in other words, follow the road along which our country is being led by the great Stalin. (*Applause.*)

Academician P. P. Lobanov. I call upon S. I. Isayev.

S. I. Isayev (Head of the Department of Fruit and Vegetable Breeding, Saratov Agricultural Institute). The remarkable development of the theory and practice of selection in the U.S.S.R. is intimately associated with the name of the eminent Soviet scientist, Ivan Vladimirovich Michurin—the founder of the Soviet school of creative Darwinism.

Darwin discovered the law of development of the organic world, and gave an on the whole correct conception of the evolution of the living beings which inhabited and inhabit the earth. But Darwin did not give any concrete indication as to how evolution can be directed so as deliberately to create new plant forms useful to man. This task of creatively developing Darwinism fell to I. V. Michurin. The Michurin doctrine is a new stage in the development of materialist biology.

"We know from science," I. V. Michurin wrote, "that all the countless species and varieties of living organisms arose by a very slow process of evolution, extending over tens and millions of years, from an original unicellular organism. . . .

"But it is possible, with man's intervention, to *force* any form of animal or plant *to change more quickly* and *in a direction desirable to man*. There opens before man a broad field of activity of the greatest value to him, namely, the improvement and creation of new forms of horticultural, medicinal and industrial plants."

Michurin provided a firm theoretical foundation for selection, whose tasks he thus defined so broadly and profoundly, and elaborated methods of directing the form-building process in order deliberately to create new forms of plants. Michurin showed that the individual development of plants may be directed, and that their hereditary nature may be modified and valuable varieties created for socialist agriculture.

It was in Soviet times that Michurin, inspired by the interest and solicitude of the great scientists and friends of science,

Lenin and Stalin, produced the best of his new varieties and summed up the conclusions of his sixty years of creative activity in works of capital importance.

After Michurin's death, T. D. Lysenko continued his work in biological science. He developed Michurin's theory and extended it to all plants, as a general biological theory of heredity and its variability. Lysenko defended Michurin's theory against the attacks and misrepresentations of the Mendelist-Morganists. He directed and united us, Michurinists, in elaborating and constructively applying Michurin's advanced theory. And now the triumph of the Michurin theory is becoming more and more apparent, for it has armed us with the best, the most rapid and effective methods of transforming the vegetable kingdom for the benefit of the working people.

In defiance of the false and essentially idealistic conceptions of the Mendel-Morgan genetics, Michurin established a correct, dialectical-materialistic approach to the study of the phenomena of heredity and variation. He looked upon the plant organism from the angle of its process of development and profound interaction with its environment.

I. V. Michurin laid the foundation of the theory of individual development of the plant organism. He established that the seedling of a cultivated fruit tree undergoes a series of changes in the course of its individual development. Not only does the outward appearance of the plant change; so also does its relation to the conditions of its environment. A young plant organism which is still in the stage of formation is distinguished by high plasticity, it is prone to be deeply affected by the influence of its environment. Hence, by creating appropriate conditions of training, deep changes may be induced in the young seedling which will also be reflected in its progeny.

Consequently, by skilfully directing the individual development of an organism, the breeder can direct its heredity, modify the hereditary properties of the organism in the direction he desires, and produce a variety with sought-for qualities.

In the past twenty years Michurin's fundamental precepts were developed in the works of Academician T. D. Lysenko, who evolved the theory of the phasic development of the plant organism. Lysenko showed that the developmental phases are

characterized and determined primarily by a succession of demands which the growing plant makes on its environment. By studying these demands we may direct the development of plants, and this is of immense importance to agriculture and plant breeding. T. D. Lysenko illustrated this by such brilliant examples as the vernalization of seed, summer planting of potatoes, and other agricultural innovations which have added millions of tons to the food resources of the country.

Taking his stand on the Michurin theory, and armed with a profound knowledge of the laws of phasic development of plants, T. D. Lysenko gave concrete examples indicating what general conditions are necessary for the remoulding of the nature of plants by means of training.

I. V. Michurin was a bold innovator who had the courage to discard scientific traditions if he saw that they hampered the advancement of science. He trenchantly criticized the "pea laws," as he called them, of Mendel, which the Mendelians proclaimed universal laws of heredity. How absurd are the attempts of the Morgan geneticists, Professors Dubinin, Altshuller and others, to adjust the Michurin doctrine to the Mendel-Morgan genetics. T. D. Lysenko, in his address, very clearly defined the basic principle of Michurin genetics, showing that it was fundamentally antithetical to the false ideas of Mendelism-Morganism, which are so pernicious to agricultural practice.

The Mendelist-Morganists hold that the conditions of growth of plants of any given variety have no influence on the alteration of its nature, on its varietal qualities. In opposition to this idea, which is so false and injurious to practical farming, T. D. Lysenko gave prominence, as the guiding principle for seed growing, to the fundamental precept of I. V. Michurin that the maintenance, as well as the improvement or deterioration, of the breed of organisms depends on their conditions of life.

Thanks to the work of T. D. Lysenko, the Michurin doctrine has been made the foundation for the reconstruction of Soviet seed raising, and this has been of great benefit to our socialist agriculture. Here again we have manifested the fundamental method bequeathed to us by Michurin, namely, that

of tackling profound theoretical problems from the standpoint of practice, and of dealing with actual problems of the day, the needs of socialist production.

It is difficult at this session to go into individual detailed questions; it is highly necessary that the Academy should in its further work draw general deductions from the creative experience of the thousands of Michurinists. However, I should like to dwell briefly on some questions arising out of my work on vegetative hybridization at the Michurin Research Institute and the Saratov Agricultural Institute, in order to show what potentialities are opened up by the application of Michurin's precepts to practical plant-breeding problems and to theoretical problems of genetics and selection. Let me say right away that our researches in vegetative hybridization were conducted in the process of handling concrete breeding assignments connected with the production of improved varieties of apple trees, with enhanced frost-resistant qualities, for the central areas of the U.S.S.R.

Vegetative hybridization, that is, the obtaining of crosses by grafting, is acquiring ever increasing importance in the theory and practice of plant breeding as Michurin's precepts are progressively mastered and elaborated.

Darwin, in his *Variation of Animals and Plants Under Domestication,* made a very thorough collection and analysis of cases of vegetative hybridization known in his time. But, Darwin wrote, we do not know under what conditions this rare form of reproduction is possible.

In Darwin's days vegetative hybrids were obtained by accident, and were therefore a rare and inexplicable phenomenon. The credit for having obtained vegetative hybrids deliberately and systematically belongs to I. V. Michurin, who made a profound study of the conditions of their formation and elaborated the mentor method, as a method of practical utilization of vegetative hybridization for plant-breeding purposes.

It is important to note that the vegetative hybrids obtained by I. V. Michurin are not mere curiosities, but economically valuable varieties. Such, for instance, is Reinette-Bergamote—a vegetative cross between apple and pear, which has been included in the list of standard fruit trees in 19 regions of the R.S.F.S.R.

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Michurin's doctrine of vegetative hybridization has been further elaborated as a result of researches made by T. D. Lysenko and the entire Michurin school, and we now have a fairly good knowledge of the major conditions needed for deliberate and systematic production of vegetative hybrids.

As T. D. Lysenko said in his address, the incontrovertible fact that vegetative hybrids have been obtained is in complete and irreconcilable contradiction to the fundamental tenets of the Mendelist-Morganists, who hastened to declare these vegetative hybrids unauthentic, or, speaking plainly, illegitimate. But Michurin in his day said that vegetative hybridization can be denied only by ignoramuses—"imitators" and "copiers," as he called them. Study of vegetative hybrids has revealed that the characters acquired by them as a result of vegetative crossing may be transmitted in propagation through seed, and, moreover, that in a number of cases that segregation of characters is to be observed which is met with in the offspring of ordinary sexual crossings.

The theory of vegetative hybridization is one of the central principles of Michurin genetics and selection, around which a battle has raged, ay, and is still raging, between the Mendelist-Morganists and the Michurinists. It would therefore not be amiss to cite one more example taken from our work on vegetative hybridization of apple trees.

Among the Michurin varieties there is the classic vegetative apple-pear hybrid, Reinette-Bergamote, which is obtained by grafting the bud of a one-year, phasically young, apple seedling onto the crown of a pear tree. For already half a century Reinette-Bergamote, when propagated vegetatively, firmly retains the character it acquired from vegetative hybridization—the pear-shaped form of the fruit near the stalk. In 1935, we, in our turn, crossed Reinette-Bergamote with various varieties of apples. Hybrids grown from seed obtained from this cross have since 1944 been bearing fruit at the experimental station of the Michurin Research Institute. And it is interesting to note that among these hybrids there are some which have inherited the type of fruit, resembling a pear, characteristic of Reinette-Bergamote; in other words, they have inherited in sexual propagation a character acquired from vegetative hybridization.

Particularly interesting in this respect is the hybrid Pepin Shafranny×Reinette-Bergamote. And it is important to stress that in order to preclude any fortuitous inaccuracies in the experiment, Reinette-Bergamote in these crossings was taken as the paternal plant.

But Michurin's theory of the mentor and vegetative hybridization has not only provided plant breeders with a highly effective method of producing new varieties; it also helps us to get a better understanding of the phenomenon of hereditary and sexual propagation, as Darwin in his time predicted.

In this connection, permit me also to dwell on the role of the maternal plant in shaping the heredity of hybrids.

Generally, this question may be formulated as follows: is there any difference between the progeny of direct crosses and reciprocal crosses; if so, what is this difference, and how is it to be explained?

From the point of view of practical plant breeding this means: is it a matter of indifference which of the members of a pair to be crossed is taken as the maternal plant and which as the paternal plant, or must a definite choice be made, bearing in mind, in the light of Michurin's teaching, the special role played by the maternal plant in heredity?

The answers given to this concrete question may be used as an example to demonstrate the impotence of the Mendelist-Morganist interpretation of the phenomena of heredity, and the creative power of the Michurin theory. What was the answer given to this question from the standpoint of Mendelism-Morganism? In his book, *Introduction to the Breeding of Agricultural Plants,* Professor Zhegalov plainly says: "From the point of view of the results obtained, it is a matter of indifference which plant is taken as the maternal, and which as the paternal."[1]

And, indeed, so it should be from the standpoint of the Mendelist-Morganist shuffling of gene factors. But this assertion does not accord with the real facts of life, with the real facts of nature, of which Michurin spoke.

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[1] С. И. Жегалов, *Введение в селекцию сельскохозяйственных растений*, 1930 г., стр. 203.

We, for example, in our experiments on apple hybrids ascertained the following: if in a cross between a northern and a southern variety, a northern frost-resistant variety was taken as the maternal plant, the offspring were more frost-resistant than in the case of the reciprocal cross, when a southern variety non-resistant to frost was taken as the maternal plant. A similar phenomenon was to be observed in apple hybrids also in respect to the size of the fruits. With one and the same pair of initial forms, the progeny as a rule bore bigger fruit when a variety with large-sized, and not small-sized, fruit was taken as the maternal plant in the pair. Other plant breeders, in the literature on the subject, have also noted that dissimilar results are obtained from direct and reciprocal crossing.

The dominating influence of the maternal plant is thus a widespread phenomenon in nature, and the practical plant breeder should give serious heed to it. This does not mean that the progeny must necessarily take after the mother, that the characteristics of the mother will necessarily dominate in the offspring. But the features and properties of a given plant will display themselves more strongly in the offspring, if this plant is taken as the maternal, and not the paternal, member in the pair to be crossed. That is why Michurin wrote that "choice of the maternal variety is in practice of exceedingly great importance."

From the standpoint of the Mendelist-Morganist shuffling of gene factors, no satisfactory explanation of this phenomenon can be given. A correct explanation of the superior role of the maternal plant in heredity can be given only from the standpoint of the Michurin doctrine, based on the theory of development, and taking account of the profound formative role of the environment, its influence on the nature of the developing organism.

The mere combination of gametes (male and female sex cells) at the moment of fertilization is in itself not sufficient fully to determine the heredity of the new organism. With regard to this fundamental question of the theory of heredity, Michurin writes the following, underscoring this passage as particularly important:

"All the distinctive characters of any variety of fruit plant is a result of hereditary transmission and a combination of the

influences of external factors, both in the embryonic period of the formation of the seed and in the post-embryonic period of the development of the seedling from the seed."[1]

What interests us in this given case is the embryonic period of development, when the embryo of the hybrid plant is forming in the fruit of the maternal plant. The embryo, organically connected with the maternal plant, and building its cells exclusively from substances elaborated by the maternal plant, must inevitably be under its profound formative influence.

The character of this influence of the adult and fully formed organism on the young organism still in the stage of formation was explained by Michurin in his celebrated theory of the mentor.

But from this viewpoint we may also regard the maternal plant as a kind of mentor, under whose influence, and at the expense of whose plastic substances, the embryo of the seed, i.e., the germ of the young hybrid organism at the most early phase of its ontogenetical development, is formed.

That in relation to the forming embryo the maternal plant must be a mentor of very great potency is a conclusion which is to be drawn from the following theoretical considerations. First, according to Michurin's theory, the younger the plant (seedling), the more plastic it is, and the easier it lends itself to the formative influence of the mentor. The developing embryo, being, as it is, in the earliest phase of its ontogenetical development, must be very easily and powerfully influenced by this formative action of the maternal plant.

Secondly, since the action of the mentor is effected through the nourishment received by the grafted plant, in the shape of the plastic substances elaborated by the leaves of the mentor plant, it follows that the action of the mentor will be stronger, the less the grafted hybrid organism utilizes the products of assimilation of its own leaves. But from this it follows quite clearly that the embryo of a seed, developing exclusively at the expense of the plastic substances of the maternal plant, must be

[1] И. В. Мичурин, *Сочинения*, том I, стр. 469.

subject to very profound formative influence on the part of the maternal plant.

Hence the dominating role of the maternal plant in heredity is explained by the fact that the embryo of the hybrid plant forming within the ovary finds itself, from the very first moment of its formation as a result of the fusion of the sex cells, under the constant influence of the maternal plant, which acts as a peculiar, powerful mentor.

In relation to plant breeding, this means that in all cases when the breeder wants to strengthen the influence of a given parent plant on the hybrid progeny, he must take this parent as the maternal, and not the paternal, component. This principle is now being utilized in the production of big-fruited frost-resistant varieties of apple trees for Siberia. Thus the Michurin theory of the mentor and vegetative hybridization helps us to get a deeper understanding of the phenomena of heredity and sexual propagation.

Permit me to exhibit certain specimens and documents illustrating what I have said regarding vegetative hybrids and the role of the maternal plants. (*Exhibits fruits, photos and drawings.*)

In concluding my speech at this notable session of the Academy, I should like to remind you of the words of Michurin, namely, that we must insistently press forward, and that every plant, even the best, must be improved and improved again. The false doctrine of Mendelism-Morganism is a stumbling block to plant breeders. But all who follow the path of Michurin may be confident that they will achieve this improvement. Thousands of Michurinists are labouring in all parts of our country, even the most remote, to produce better varieties, worthy of the fields and orchards of the great land of Socialism.

Every one of us knows that we owe all our achievements in plant breeding to the creative virtue of Michurin's teachings. And every one of us remembers the command of our teacher, those proud words of Michurin which are engraved on his monument:

"Man can and must create new breeds of plants better than Nature." *(Applause.)*

Academician P. P. Lobanov. I call upon Academician N. G. Belenky.

Academician N. G. Belenky. The President of our Academy, Academician T. D. Lysenko, gave in his programmatic address a strictly objective and scientific analysis of the state of Soviet biological science. He showed that the more profoundly biological science discloses the laws of living bodies, the more effectual agronomical science becomes. The rapid development of socialist agriculture and stockbreeding in the U.S.S.R. makes it incumbent on us to penetrate deeper and deeper into the laws of development of living beings.

The only correct theory, the one capable of illuminating the path of practical agronomy, is the Michurin-Lysenko theory, that creative development of Darwinism which generalizes and further advances all the best that has been accumulated by science.

That the Michurin doctrine should have arisen in our country is not fortuitous, but quite natural, since in our Soviet land, with its progressive, revolutionary ideology and advanced agriculture, all the necessary conditions exist both for the tackling and for the correct solution of scientific problems, including those connected with the discovery of methods of directing the development and heredity of agricultural animals and plants.

We know that nowadays Soviet Darwinism, the new Soviet biology, is attacked by reactionary foreign biologists, as well as by certain scientists in the U.S.S.R., who call themselves adherents of classic genetics. The foundation of this classic genetics is, as you heard in the address, the theory of a hereditary substance. The adherents of this formal trend in biology believe that this specific hereditary substance is transmitted from parent to progeny through the reproductive cells in the shape of minute particles, or genes, which determine the reproduction in the progeny of characteristics identical with the characteristics of the parents.

A trend has of late arisen in the camp of the Morganist-Mendelists which assumes that, apart from the genes localized in the chromosomes (the chromosome theory of heredity), there are genes localized in other parts of the cell (plastid genes, or

plasmogenes). This addition makes no difference in principle since it does not alter the essence of the theory, the theory, namely, of a specific, unchangeable hereditary substance.

Speaking of these additions, it should be mentioned that quite recently Docent Alikhanian, acting director of the Department of Genetics of the Moscow University, in defiance of common sense and the elementary principles of the physiology of the endocrine system, was disposed to regard genes almost as internal secretion glands. This postulate is as scientifically frivolous and sterile as the pretentious boast he made, during the discussion on biology organized by the journal *Pod Znamenem Marxisma* (*Under the Banner of Marxism*) in 1939, that on the basis of the Mendel-Morgan theory he had developed a new breed of chicken.

Creative Darwinism, as developed by Academician T. D. Lysenko, categorically denies the theory of a hereditary substance, as being metaphysical and contrary to experimental data.

A specific hereditary substance no more exists than does phlogiston, as the substance of fire, or caloric, as the substance of heat.

With the theory of a hereditary substance is inseparably associated the concept of the continuity and independence of the germ-plasm, according to which the germ (reproductive) cells of animals are derived, not from the cells of the animal body in which they developed but directly from the germ cells of the preceding generation.

As the Weismannists (Mendelist-Morganists) conceive it, the body of an organism is only the receptacle and source of nourishment of the continuous germ-plasm. "In origin," writes T. H. Morgan, "*they* [the germ cells] *are independent of the rest of the body and have never been a constituent part of it. . . .* They are protected and nourished by the body, but not otherwise influenced by it."[1]

Morgan's follower, the American, Castle, explains: "In reality the parent does not produce the child nor even the reproductive cell which functions in its origin. The parent is himself merely a byproduct of the fertilized egg (or zygote) out of which he arose. The direct product of the zygote is other reproductive

[1] T. H. Morgan, "Heredity," *The Encyclopedia Americana*, 1945.

cells, similar to those from which it arose. ... one being the immediate and direct product of the other."[1]

He is echoed by M. M. Zavadovsky—who yearns for an "international language within the framework of biology"—in his textbook for university students, *Dynamics of Development of Organisms*. He deems it necessary to "join with Nussbaum who maintains that sexual products do not develop from the maternal organism, but from the same source as the latter," and that "the seminal corpuscles and eggs do not originate in the parent organism, but have a common origin with the latter.... The germ cells and the cells of the soma should be regarded not as daughter and parent generations, but as twin sisters, of which one (the soma) is the feeder, protector, and guardian of the other."[2]

What we are thus told is that children are not the offspring of their parents, but only their younger sisters and brothers, and arise with them from one and the same principle.

The Michurin doctrine, as Academician T. D. Lysenko stated in his address, rejects this pseudoscientific conception of the origin of the reproductive cells lock, stock and barrel. Soviet Darwinism upholds the view that the sex cells are formed as a result of the development of the living body, and that within them, as Academician Lysenko writes, "is, as it were, accumulated the whole course of development traversed by the organisms of preceding generations." Hence the changes produced in the parents by the action of changes in the conditions of their existence result in a responsive change in the nature of the reproductive cells.

Academician T. D. Lysenko holds that the material vehicle of heredity is the living matter in every cell. Every particle of the body that is capable of absorbing nourishment, of growing and multiplying, in other words, which possesses the fundamental properties of life, also possesses the property of heredity. Changes in the heredity of a living organism are a consequence of changes in the developing body of the organism itself, and not of changes of some specific hereditary substance. Changes of heredity always proceed through changes in the process

[1] W. E. Castle, "Genetics," *The Encyclopedia Americana*, 1945.
[2] М. Завадовский, *Динамика развития организма*, 1931 г., стр. 321.

of development of a living organism, under the influence of changes in its conditions of life, in the conditions of nourishment of the living organism. The sex and vegetative reproductory cells which are formed in the changing body of a living organism are, like all other parts of the body, connected with the entire organism by the process of metabolism. They may therefore (although not always, nor in equal degree) undergo corresponding changes in response to the influence of changes occurring in the body of the developing organism.

The Mendelist-Morganists deny that acquired characters, changes in the body of an organism caused by the action of the conditions of the external environment, can be inherited. They attribute every proof of the inheritance of acquired characters to a mistake in the experiment, and class its author as a Lamarckian.

The Michurin doctrine rejects the Mendel-Morgan postulate that hereditary characters are independent of the conditions of life of the animal, and recognize the possibility of obtaining directed variations of heredity under the influence of factors of the environment, by training the organism in definite conditions.

The view of the adherents of the Michurin theory is firmly corroborated by numerous facts. There are, for instance, the successful experiments in deliberately changing winter plant varieties into spring varieties, and vice versa (spring into winter), by training the plants in suitably changed conditions of life; the successful experiments in producing plant hybrids by grafting, which proves the possibility of uniting the heredities of two plant organisms in one hybrid organism without uniting the chromosomes of the parent (i.e., of those parts of the cells where the corpuscles of the specific hereditary substance are supposedly located); the phenomena of vegetative segregation, which point to the possibility of a disjunction of the parental characters in the vegetative progeny of hybrids, that is, when there is no disjunction of the chromosome pairs. Among facts of this sort may be classed the experimental studies of the elective character of fertilizations, which show, on the one hand, that it is possible to obtain heterogenous progeny from homozygous plants by self-pollination and, on the other, that it is possible to preserve the varietal type and homogeneity in

the progeny of cross-pollinating plants obtained by open intervarietal cross-pollination, that is, homogeneity when the progeny is clearly of hybrid origin. Here, too, belong the facts which demonstrate the correctness of Darwin's view that a character which has begun to vary in a definite direction, continues to vary in the progeny in the same direction, if the conditions which caused the variation originally to appear in the ancestors continue to act upon the progeny.

All the facts connected with the production of new breeds of cattle also incontrovertibly prove the correctness of the Michurin view.

By attaching prime importance to feeding and maintenance in producing a desired type or breed of animal, Soviet zootechnicians have succeeded in creating valuable cattle breeds in relatively short periods of time. The high-yielding Kostroma breed of cows was obtained in this way. In this way, too, was produced a fine-fleeced breed of sheep well adapted to all-the-year-round pasturing in the conditions of Kazakhstan and yielding a big clip of fine wool. It was also in this way that the Aulie-Ata breed of cows was created in Kirghizia, and so on.

It is only from the standpoint of the Michurin doctrine that highly productive breeds of animals corresponding with the requirements of the national economy are being bred, and will be bred, in our country.

The Mendelist-Morganists have not yet produced a single new breed of animal, although extensive opportunities have been placed at their disposal for their work.

During the discussion in 1939 I had occasion to speak of the immense harm which the Mendelist-Morganists had caused, and are causing, to the breeding of pedigree cattle.

In the opinion of the Weismannists, the whole work of breeding highly productive animals amounts to "enriching" the animals with genes of high productivity, which supposedly remain unchanged during periods of food shortage and reappear again when the conditions become favourable. From the standpoint of this "theory," covering our best native stocks with other stocks is the only method of improving local breeds. It is generally known what immense harm has been done to sheep breeding by the method of testing rams for population while ignoring proper selection of the ewes, and so forth.

The Morganists regard the feeding and maintenance of cattle as only a background, against which the action of the genes manifests itself, and not as factors in the active creation of high-produce animals and in modifying their heredity in desired directions.

Every passing year convinces us more and more of the harm which Mendelism-Morganism is causing to the development of animal husbandry. It is noteworthy that progressive scientists and practitioners in other countries are coming to similar conclusions. A. Fraser, for instance, writes in the English magazine, *Farmer and Stock-Breeder* (1947, Vol. 61): "We have, in this country [Great Britain], produced great and useful breeds of both cattle and sheep, indeed of all kinds of farm livestock. Let it be at once admitted that genetical science has played no part whatever in their differentiation and establishment." "More seriously," Fraser adds, "the science of genetics [he means Mendelism-Morganism] has not yet the practical achievements to its credit. . . ."[1]

Fraser says that in the U.S.A. for already thirty-three years, that is, since fat-rumped sheep were first introduced into America from Siberia in 1914, the Mendelist-Morganists have been trying, but without success, to create a breed of sheep without tails. He ironically concludes: "We might find ourselves saddled not merely with a sheep without a tail, but with a tail without a sheep."

It must be said that for biologists in general and for us, physiologists, in particular, the very conception of the living organism is of exclusive and fundamental importance, since it determines what direction should be given to investigations of the functions of the organism in general and of its separate parts.

The Mendelist-Morganists, when defining the living organism, do not include the environment which forms and determines its functions. They divorce the organism from its environment. Academician T. D. Lysenko hotly protests against this conception of the organism; he regards the organism and the environment it inhabits as an integral whole. In this, he adheres to the view of the organism expounded by that brilliant scientist, I. M. Sechenov, the father of our Russian physiology. In his

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1 *Farmer and Stock-Breeder*, Dec. 2, 1947, p. 2741.

article, "Two Concluding Lectures on the Significance of So-Called Vegetative Acts in Animal Life," published in the *Meditsinsky Vestnik,* 1861, No. 26, Sechenov says: "An organism without an external environment supporting its existence is impossible; hence the scientific definition of an organism should include the environment which influences it, since without the latter the organism cannot exist."[1]

A definition of the organism which includes in the very concept organism the milieu in which it exists, gives the researcher and the practical breeder a correct orientation, permitting them to work creatively in the production of new forms and in directing the vital processes of the organism.

I have not the opportunity in this speech to describe in detail the numerous direct experiments proving the view of the Michurin theory, creatively developed by Academician T. D. Lysenko, on the inheritance of acquired characters of animals resulting from the action of the factors of external environment. There are many such experiments. I shall dwell only on a few. Let us recall the well-known experiments of Kammerer.

Kammerer used for his experiments two species of closely related salamanders: the yellow-black spotted *Salamandra maculosa* and the black alpine *Salamandra atra.* The latter is normally viviparous: it bears two salamanders, already fully formed and adapted to terrestrial life, 38-40 mm. long, which have passed through all the stages of metamorphosis within the maternal organism.

In contradistinction to *Salamandra atra, Salamandra maculosa,* which normally inhabits humid forests, is simultaneously viviparous and oviparous. It is capable of bearing a large number of incompletely formed aquatic larvae 25-30 mm. long, with four extremities and short gills, or of laying large eggs in water, from which emerge similar larvae, 23-25 mm. long.

Both these kinds of larvae, after several months of life in water, undergo a complete metamorphosis and are converted into terrestrial salamander, 45-50 mm. long. .

Kammerer, in his experiments, tried to change the method of propagation of both these species of salamander by changing

[1] И. М. Сеченов, „Две заключительные лекции о значении так называемых растительных актов в животной жизни", *Медицинский Вестник*, 1861 г., № 26.

their conditions of habitat. He kept spotted salamanders without water, as a consequence of which the eggs they laid and the larvae they bore perished.

But after a while the salamanders began to retain the eggs and the embryos within their organisms until they had undergone the complete metamorphosis, after which they were born already adapted to a terrestrial mode of life. As a result of "educating" them in conditions where water was absent, the spotted salamanders began in many of their features to resemble black salamanders: 1) the number of progeny they bore sharply decreased (2-7 at each birth, not more); 2) the behaviour of the eggs and embryos before birth began to resemble the behaviour of those of the black salamander, and 3) the colouring of the young changed until it was almost black.

The progeny of the "re-educated" spotted salamanders obtained by Kammerer in this experiment, after they had reached sexual maturity, were again given access to water. Although they were now in conditions normal for the spotted salamander, these progeny nevertheless continued to a certain degree to retain the altered mode of propagation: they laid no eggs at all, but gave birth to aquatic larvae characterized by strongly reduced or rudimentary gills, and the progeny at the moment of birth were at a more advanced stage of metamorphosis than is normally observed in the spotted salamander.

Kammerer treated the black alpine viviparous *Salamandra atra* in the same way. He began to train these salamanders in conditions of high temperature and abundance of water, and likewise achieved a change in their mode of propagation. Instead of two fully formed young, prepared for terrestrial life, they began to produce from 3 to 9 aquatic larvae, which underwent their metamorphosis in water.

Kammerer continued to keep the progeny of the "re-educated" black salamanders under the conditions of the basic experiment (high temperature and access to water). The modification of the parent individuals was fully retained in them, or was even intensified: they also bore 3-5 larvae (21-23 mm. or 33-44 mm. long), of a light colour and possessing gills.

In another series of experiments with the spotted salamander, Kammerer set out to alter the colour of their bodies by adaptation. They have irregular and variable yellow spots scat-

tered over their black bodies. For several years Kammerer kept the darker variants on a yellow background, and the yellower variants on a black background. The former betrayed a noticeable increase of the yellow hue in the yellow spots (they became brighter), while the latter, brought up on a black background, betrayed a darkening in colour.

The progeny of the salamanders which revealed an increase in yellow areas when kept on a yellow background, were divided by Kammerer into two groups: the first he continued to keep on a yellow background, while the second were placed on a black background. In the first group the size of the yellow areas increased immensely, while in the salamanders of the second group the yellow pigmentation became less than in the first, but they were nevertheless considerably yellower than in the normal spotted salamanders, in spite of the fact that they were reared on a black background. It may be concluded from this that there were transmitted to them by inheritance characters acquired by their parents under the influence of factors of the external environment.

Guthrie performed the following experiment on hens: he transplanted the ovary of a white hen to a black hen, and the latter was fertilized by a white cock; but the chickens were partly white (9), and partly speckled (11). Similarly, a white hen to which the ovary of a black hen was transplanted and which was fertilized by a black cock, yielded speckled chickens (12); since the heredity of both the male and female organisms in both cases was similar, the appearance of the black colour in the first case and of the white in the second can be explained only by the influence of the somatic cells on the sex cells.

Guyer and Smith introduced into the organism of she-rabbits antibodies of the cornea (cytolytic serum which destroys the substance of the cornea of rabbits' eyes). The antibodies had no apparent effect on the formed organism of the mother rabbits, but their young in some cases betrayed defects of the eye (filming of the cornea, complete disappearance of the iris, etc.). These anomalies were, without further outside interference, transmitted by inheritance to the offspring for nine generations, and moreover in each succeeding generation the defects were observed to be more strongly marked—and this without any additional interference. The anomalies of the eyes were trans-

mitted not only through the mothers, but also through the fathers (when a male with the anomaly was mated with a normal female). These experiments were corroborated in subsequent tests.

Griffith induced in rats a hereditary specific disturbance of equilibrium (vestibular defect), by causing the parental pairs over a period of up to one and a half years constantly to revolve in round cages either in a clockwise or an anti-clockwise direction. This defect of the semi-circular canal was transmitted by inheritance.

Quite recently, S. Russ and J. M. Scott published experiments on rats which indicate that immunization of parents against Jensen sarcoma is to a certain degree transmitted to offspring in the shape of a partial resistance to cancer injections. The transmission took place not only through the mothers, but also through the fathers. When the young of immunized rats were injected with sarcoma, the average size of the tumours formed was almost half as small as that in the control rats. Futhermore, dissipation of the tumours was more frequent in the former than in the controls (42% of cases, against 10%). A similar result was obtained when sarcoma was injected into young rats whose parents had not been themselves immunized but had only been born of immunized animals. Resistance to sarcoma caused by immunization was transmitted to the second generation of progeny without further treatment.

Bloor established that changes of properties of muscles caused by exercise can be transmitted by inheritance.

For more than a month he caused she-rats to exercise in special cages. The offspring, upon attaining a weight of 120 g., were also put in cages designed to exercise the muscles. Half the offspring were killed for the purpose of analysis, the other half were used for propagation. This was repeated with the next two generations.

The individuals of the second and third generations were found to have more highly developed muscles than the rats of the first generation (with a comparatively small difference between the rats of the second and third generations). At the same time, the muscles of the rats of the second and third generations contained considerably more phospholipides, and especially cholesterol, than those of the first generation.

From this Bloor concludes that the effects of exercise of the muscles are transmissible to offspring.

I shall not continue the large list of experiments on animals, and shall only say that I recently had occasion to verify and somewhat extend the old experiments of Brown-Séquard on guinea pigs. Severance of the sciatic nerve at the place where it emerges from the spine leads to the formation in the animal of what we have conditionally designated as an epileptogenic zone, irritation of which often leads to tonic contraction of the muscles. This characteristic, caused in the parent animals by surgical interference, we discovered in individual specimens even in the third generation. More, in that part of the guinea pig progeny in which the reaction resembling epilepsy was not detected externally, we observed obvious deviations from the normal in the character of the excitability of the nervous and muscular system which are not manifested externally. This we ascertained by determining the time of the reflex and the threshold of excitability of the rear extremities of the guinea pigs.

Thus Brown-Séquard's experiment has been confirmed many decades later. The conclusion to be drawn from it is fairly obvious, namely, that characters acquired by the animal organism under the influence of factors of the external environment can be inherited. This conclusion is absolutely inacceptable to the Morganist-Mendelists, for if they recognized it they would have completely to discard their theory, since the cornerstone of their "doctrine" is that characters acquired by an organism are not heritable.

I should like to draw your attention to another thing. Until now, the Mendelist-Morganists in our country have had the widest opportunities in the matter of educating our youth, our Soviet biologists, future specialists in agriculture. It is hard to find any justification for the consideration which the Mendel-Morgan genetics and its exponents enjoy in our higher educational establishments (especially the universities).

Textbooks and manuals for higher and intermediate educational establishments are crammed with the ideas of reactionary Mendelism-Morganism, which incorrectly orientates the future agronomists in cardinal questions of agriculture.

Academician T. D. Lysenko has already spoken of the textbook of Sinnott and Dunn. All he has said fully applies to other

manuals, especially to the textbooks on which our future zootechnicians are educated. I cannot help referring to Professor Rokitsky's textbook on genetics. This book has unfortunately been accepted as the chief textbook for zootechnical colleges. In it, in the most unconcealed form, reactionary Weismannism is represented to students under the guise of Darwinism. And how could the author of the book, Professor Rokitsky, behave otherwise, when he asserts that, just as property—clothing, weapons, domestic utensils—were handed down from father to son, so a number of characteristics of great-grandfathers, grandfathers and fathers (height, eye colour, colour of the hair, certain features of the face) were handed down to children and grandchildren. With regard to such views, comment, as the saying goes, is superfluous.

Let us take another example. This is Professor S. G. Davydov's manual, *The Breeding of Agricultural Animals*. Professor Davydov's manual is entirely built on the Mendel-Morgan theory of heredity. The author does not make the slightest attempt critically to analyze the suitability of the principles of this theory from the point of view of practical breeding.

On the contrary, the erroneous dogmas of this theory, which are so harmful to practical work, are regarded by the author as the most amazing achievements of recent years, opening up broad perspectives for the improvement of agricultural animals, such as we did not even dare to dream of formerly. We can really assume, Davydov writes, that the time is not far off when we shall be able to obtain any combination of heritable tendencies we require.

The author, who in this textbook fully espouses the views of Mendelism-Morganism, in point of fact denies the creative role of selection, and reduces breeding simply to recombinations of ready-made and practically unchanging genes.

It follows from what this author says that the breeder cannot, by selection and training, elaborate in the selected animals new characteristics and qualities which were not extant in the initial parents, but can only reshuffle and recombine in the progeny the qualities and characters possessed by the parents.

Taking his stand on this anti-Darwinist position, the author deliberately ignores, as something antiquated, the experience of the classic breeders, which demonstrates the creative, construc-

tive role of artificial selection. In this textbook he passes over in silence the experience of P. N. Kuleshov, M. F. Ivanov and other of our outstanding Darwinist breeders.

He says practically nothing about the methods and eminent achievements of such a brilliant scientific breeder as M. F. Ivanov. But at the same time, he details at length the views and theoretical tenets of such absolutely barren workers in the field of breeding as Professor A. S. Serebrovsky and his collaborators.

The material in the book has been chosen one-sidedly, with a bias for the Mendel-Morgan theory of heredity. Everything that contradicts this theory is discarded by the author. One example of this one-sided and biased selection of material is the section of the book devoted to inbreeding. The author denies the Darwinian view regarding the perniciousness of close breeding and the usefulness of crossing, and makes no mention of the experimental data which substantiates this view. Nor does he say anything about Darwin's indications regarding the ways in which the pernicious effects of inbreeding can be mitigated. In general, this book, instead of an objective, scientific analysis of the achievements of advanced animal breeding practice, treats the reader to erroneous views, which are contradicted by the whole experience of advanced breeding practice and which, far from helping Soviet breeders and zootechnicians to cope with the problems confronting them, are only capable of disorientating them as regards the proper methods of solving these problems.

Such, or roughly such, is the position in respect to the other textbooks recommended for universities and colleges.

It is absolutely incomprehensible why the new, Soviet, Michurinian genetics, the scientific foundation of all the animal and plant-breeding work done in our country, is almost totally ignored.

I cannot refrain from telling you of a curious incident which occurred in our college. Docent Platonov, who was originally recommended by Professor Zhebrak, delivers his course to his students in a manner which caused the students to protest. Thereupon, the dean of the faculty, Professor Ogulnik, asked Platonov to come and see him and requested him to revise his position. In reply, the director received a lengthy

missive from Platonov, in which he wrote in substance as follows: I shall give my course as I gave it hitherto, and just try to stop me!—this is the way it is given by Academician Schmalhausen, Zhebrak and the others.

These people inculcate a profound hostility to Michurin, Lysenko and the Michurin theory. Boris Mikhailovich Zavadovsky, at the Moscow City Pedagogical Institute, particularly distinguishes himself in this respect. So say the students and collaborators of Boris Mikhailovich and even leading officers of the Institute.

Energy is spent on training and educating students in a spirit of deep hostility to the Michurin genetics, which is represented as a "naive delusion" on the part of I. V. Michurin and T. D. Lysenko and as a "delusion" on the part of all who share the views on heredity of Darwin, Timiryazev, Sechenov, Burbank, Daniel and all the best Darwinian biologists.

Is this the fruit of a thoughtless attitude towards the essence of the divergencies between the Michurin genetics and Morganism-Mendelism, or is it the fruit of a rotten liberalism? I think the latter. It seems to me that the time has come to put an end to the extravagant propaganda of the metaphysical doctrine of a hereditary substance, which has been discredited in practice and is reactionary in its very essence.

The time has come when in our universities and colleges the door must be thrown wide open to Michurin's teachings. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician P. N. Yakovlev.

Academician P. N. Yakovlev. At this session I shall dwell briefly on the subject of vegetative hybridization of plants, which was touched upon by T. D. Lysenko in his address. This question is of vast and fundamental importance for the progressive development of our Soviet agrobiological science. Nowhere in the world has the doctrine of vegetative hybridization been taken up and raised to such heights as in the U.S.S.R.

This is not a new question. We find Darwin in his time referring to many facts borrowed by him from practical

horticulturists and communicated to him by experimenters. But the doctrine of vegetative hybridization was taken up practically and profoundly elaborated in its theoretical aspects by our fellow-countryman, the great Russian scientist, I. V. Michurin.

Whereas Darwin, in his immortal writings, gives only fragmentary data on vegetative hybridization, Michurin applied it in his work and by it produced a number of economically valuable varieties for our practical horticulture. S. I. Isayev has already referred here to the apple variety Reinette-Bergamote which has been introduced into the standard assortment of 19 regions of the Soviet Union. Mention should also be made of other varieties obtained by Michurin by means of vegetative hybridization. These are: among the apples, Kandil-Kitaika, and Bellefleur-Kitaika; among the plums, Sweet Blackthorn and Blackthorn Reine Claude; among the cherries, Krasa Severa; among the pears, Bergamote Novik, etc.

These varieties were produced by Michurin not for exotic effect, not by way of subtle, complex and masterly experiment for experiment's sake. No, he deliberately set out to create by means of vegetative hybridization varieties for production purposes, to multiply them as standards for many regions of the Soviet Union. It is worth mentioning that the splendid autumn variety, Bellefleur-Kitaika, which Michurin produced by the method of vegetative hybridization, has now been accepted as standard and is propagated in 44 regions of the U.S.S.R.

By his doctrine of vegetative hybridization of plants, which was founded upon numerous facts and the precise observations of a born naturalist, Michurin added breadth and depth to Darwin's theory that the variability of organisms, whether produced sexually or asexually, vegetatively, is governed by the same laws, and that there is no great or fundamental difference between the sex cells and the body cells.

The sex cells, in the final analysis, are formed at a definite stage of development of the organism from the somatic cells of which the whole organism consists. The sex cells build their bodies, as Academician T. D. Lysenko figuratively puts it, from the "living" organic food provided by the vegetative

cells. Hence the closest interaction exists between the vegetative and the sex cells.

In the early stage of the embryonic and post-embryonic development of the plant organism, we are unable to observe any clear distinction between the somatic and the sex cells. Hence all changes in the living conditions to which the organism is relatively adjusted must undoubtedly be reflected in the mechanical and biochemical structure of its cells. Up to a certain limit, these cells rearrange their reactions to the various influences of the external environment, causing ontogenesis to proceed in a different way, and embracing one or other stage of its development. As a consequence, the structure of its sex cells must inevitably change likewise.

To us, practical agronomists, the work of the eminent naturalists, Darwin and Michurin, furnish a clear and incontrovertible answer to the question which is of late the subject of such fierce controversy between representative scientists of the two trends—those who hold the position of bourgeois formal genetics, and those who take their stand on the effective materialist views of Darwin, Timiryazev and Michurin, whose teachings are being so brilliantly developed in our day by Academician T. D. Lysenko.

Some of our geneticists who hold the formal position absolutely deny vegetative hybridization. In their opinion, no such thing as vegetative hybridization exists.

I have time and again heard Professor Zhebrak declare that hybridization can only be sexual, and not vegetative. The adherents of formal genetics used until quite recently to say to us, the adherents of the Michurin school: Give us facts showing the inheritance by progeny of characters derived from vegetative hybridization, and then we may be won round to your conception that no great or fundamental difference between sexual and vegetative hybridization exists.

In the past nine or ten years, numerous experimenters working under the guidance of Academician T. D. Lysenko in various parts of the Soviet Union have done splendid work on the vegetative hybridization of annual herbaceous plants with sharply contrasting features. In these nine or ten years this school has accumulated more data on vegetative hybridization than had been accumulated all over the world in the

past 150 years. It has been demonstrated that there is a powerful reciprocal influence between the graft components, and, moreover, that the changes obtained have been firmly inherited, not only by a series of vegetative but also of seed generations. For example, Dr. I. E. Glushchenko describes vegetative hybrids of tomatoes with altered fruit colouring, and the character of these alterations, down to the fourth seed generation.

More than twenty years ago, long before the heated dispute between the followers of the two trends in modern genetics flared up, Michurin crossed the vegetative apple-pear hybrid which he called Reinette-Bergamote with various southern varieties of apple—Bellefleur Zholty, Kandil-Sinap, Golden Pearmain, and others. It was Michurin's purpose, in making these crossings, to provide graphic proof to the disbelievers in vegetative hybridization and to convince them that characters obtained by vegetative hybridization are also transmissible when propagated by seed, and not only in the case of herbaceous plants, but also in the case of perennial dendrous plants.

Some of these hybrid seedlings first began to bear fruit in 1946 and 1947. The correctness of Michurin's views and his brilliant gift of scientific prediction were then demonstrated in convincing form: the majority of the fruit-bearing seedlings, obtained from the vegetative hybrid Reinette-Bergamote and fertilized with various southern apple varieties, have firmly inherited the characters derived by this hybrid from the pear as the result of vegetative hybridization. The colourful picture presented by these exhibits graphically confirms this. (*Exhibits drawings.*)

An analogous picture was observed subsequently in the seed generations obtained by S. I. Isayev from crossings of the Michurin apple varieties, Slavyanka and Pepin Shafranny, with this same vegetative hybrid, Reinette-Bergamote. In the latter case, this variety was taken, not as the maternal component, as in the work of Michurin, but as the paternal component.

Facts obtained by many experimenters in different parts of the Soviet Union graphically demonstrate how mistaken are the Weismann-Morganists, inveterate representatives of whom, among others, are Academician Schmalhausen and Professor

Dubinin. Without verification, and without any proof, they irresponsibly deny the numerous facts of the inheritance of characters by progeny in the case of propagation by seed of plant organisms, both herbaceous and perennial dendrous fruit plants, which were obtained by vegetative hybridization.

In the work on directing the development of plants with the help of mentors, interesting facts have been obtained by many experimenters in our country. I shall not dwell on them, but I must say, on the basis of my many years of work, that the leaf apparatus—which performs a most important physiological process in the organic world, namely, photosynthesis—is a decisive factor in the synthesis of the various groups of proteins and carbohydrates which determine the specific or generic features of the different species of plants. The leaf system of the mentor, which trains the young hybrid that has not yet passed through all the phases of its ontogenesis, exercises of course a radical influence on the protein-carbohydrate composition of the hybrid, causing it to undergo deep changes.

The huge leaf system of the mentor—which can be regulated by pinching the leaves, cutting back shoots, etc.—provides its specific organic food in abundance to the young hybrid, whose hereditary characters are not yet fixed, through exceedingly fine protoplasmic threads—the plasmodesms. The latter connect the cells one with another and effect the single physiological process of assimilation and dissimilation in the multicellular organism, radically changing the biochemistry of the young hybrid organism, in an adequate direction, which is undoubtedly reflected in the reproductive sphere of the hybrid. This view is graphically confirmed by the work of Academician T. D. Lysenko and many other Michurinists.

It may be seen from the experiments that, under the influence of the mentor, not only do the biochemical composition of the cells and the size and colouring of the fruits change; so also does the shape of the fruits of the hybrid trainee, which in most cases inclines towards that of the mentor.

The transmission from mentor to hybrid of its biochemical properties, and the size and colouring of the fruit is comparatively easy to explain; but to explain the transmission of the shape of the fruit from the mentor to the hybrid is exceedingly

difficult. Surely, the genes, or any other "hereditary substance," cannot transmit through space, as it were, the character of shape borrowed from the stock or scion taken for the vegetative hybridization.

At any rate, much work has still to be done on this question. Evidently, special experiments will have to be undertaken, enlisting experts in other branches of botany in this work, in order to shed light on this interesting, but at present highly inexplicable, feature of the observed phenomena.

Michurin's doctrine of vegetative hybridization of plants is being developed with great rapidity by progressive scientists in our country who proceed from the materialist standpoint, the only correct and effective standpoint. In this respect, Michurin's services to biological science are immortal. The ways of deliberately and systematically changing both plant and animal organisms have been mapped out by the great Russian scientist, I. V. Michurin. It is by following these ways that the talented continuer of Michurin's work in our country, Academician T. D. Lysenko, has made such great advances, and it is by following these ways that further achievements will be registered by our Soviet, Michurin agrobiological science. (*Applause.*)

Academician P. P. Lobanov. I call upon Comrade P. F. Plesetsky.

P. F. Plesetsky (Director, Ukrainian Scientific Research Institute of Fruit Growing). Academician T. D. Lysenko demonstrated in his address the existence of two diametrically opposed trends in contemporary biology. He revealed and described with the greatest clarity the philosophical roots of the two trends.

One of them, the idealist, Mendel-Morgan trend, is effete in the cognitive respect and sterile in the practical respect. It is characteristic of the representatives of this trend that they disregard the requirements of our people and associate themselves with reactionary scientists abroad. The roar of guns on the battlefields had not yet ended, the loyal sons of the Soviet people were still shedding their blood in defence of our Country's honour, freedom and independence, and the toilers at

home, while helping the front, were at the same time rebuilding wrecked towns and villages, factories and mills; but representatives of the Mendel-Morgan trend in biology, like Professor Dubinin, were busy solving so extremely "important" a problem as ascertaining the number and ratio of the fruit-fly population that perished in the city of Voronezh which suffered destruction at the hands of the German invaders. This is not a lyrical digression from the academic style of speeches; I am mentioning this because it is characteristic of the nature and style of the work of a Mendelist-Morganist. The war had not ended when the fomentors of a new imperialist war appeared on the political scene in the capitalist countries, primarily in Great Britain and the United States. Among them were Sax, Darlington and other representatives of Mendelism-Morganism. Yet Professor Zhebrak, in an article printed in the magazine *Science* in 1945, asserted that he was building up "a common, world-wide biology" together with those reactionaries in science. This, too, is not a lyrical digression, but a fact characteristic of the political physiognomy of a Mendelist-Morganist.

The other trend in biology, the Michurin, materialist trend, is of vast cognitive value and has a profound influence on research, lending it purpose productive of practical results. I. V. Michurin possessed an invaluable treasury of views and methods of investigation which enabled him to regulate the development of vegetable organisms and, on that basis, to create a large number of agricultural plant varieties, renovating the wealth of fruit strains and the varieties of other plant cultures and extending fruit growing far to the north.

The effectiveness of this or that view in science is judged on the basis of its practical worth. Judging Mendelism-Morganism and the Michurin teaching from this point of view, we come to the conclusion that, far from helping to cope with practical problems of socialist agriculture, Mendelism-Morganism, owing to the harmful idealist views of its proponents, hampers the solution of such important problems. The Michurin teaching, on the other hand, which has a mass following, has proved to be of great assistance to socialist agriculture.

Michurin's numerous disciples and followers, first and foremost Academician T. D. Lysenko, the most eminent representative of the Michurin trend in biological science, who has

made an invaluable contribution to the further development of this trend, are elaborating methods to regulate the development of vegetable and animal organisms, creating new, more productive varieties and breeds, introducing new scientific methods in agriculture and animal breeding, raising the productivity of plants and animals.

I take this opportunity to give a brief description of some sections of the work of the Ukrainian Scientific Research Institute of Fruit Growing. This is all the more necessary since the activity of the Institute may be divided into two periods distinguished by diametrically opposing trends.

In the first period many of the leading workers of the Institute conducted their investigations from a Mendelist-Morganist standpoint. They ignored Michurin's teaching, and the results of the Institute's work in that period are extremely meagre.

The second period began with a change in the trend of investigations. Michurin's ideas were accepted as the ideas by which the Institute was to be guided in its work, and the results are highly successful. Since I cannot here give a full account of the Institute's work, I shall deal with only a few points to give you an idea of the style of its researches and their results.

The Institute has been guided in its breeding work by I. V. Michurin's and T. D. Lysenko's findings that the qualities of hybrid seedlings, the parents of future varieties, are formed by the action of the conditions of their existence. By following these findings, the Institute has been able to produce outstanding varieties of fruits and small fruits. Nine of them are already grown in definite districts and hold an important place among the fruits produced in the Ukraine. Some dozens of new forms have been included in the state testing program and are also undergoing tests at breeding stations. Before long these new forms will have eliminated the defects in the assortment of fruits and berries in the Ukrainian S. S. R.

It is very rare for a plant breeder to obtain by crossing a form which meets the demands put to the new variety. I. V. Michurin said that the hopes to obtain new varieties by such means may be likened to treasure hunting. To obtain a variety with the required qualities, the breeder must active-

ly intervene in the process of formation of the hybrid seedling.

The active role of the breeder in this matter may be traced, for example, in the production of the new variety Pervomaiskoye apple, new varieties of pears and other fruits.

The seedling of the apple which later became the Pervomaiskoye was obtained by crossing Champagner Reinette with Landsberger Reinette. It had many good qualities: late maturing, high winter hardiness, firm pulp. But its flavour was flat. In order to improve the flavour, seedlings of Golden Winter Pearmain were used as mentor, with the result that Pervomaiskoye has acquired a good flavour and is now grown as a first-grade variety.

The pear is considered a "difficult" breed for the production of new varieties. But the difficulty is only seeming. The methods of training suggested by I. V. Michurin make it possible to produce new varieties of pears as successfully as other fruit varieties. The only difference is that pear seedlings are more labile than, say, apple seedlings, and in order to obtain new pear varieties it is necessary, besides selecting the proper initial forms, to pay the strictest attention to the individual properties and requirements of the hybrid seedlings. Even a general analysis of hybrid seedlings of the pear as a group reveals the necessity of strict attention to their characteristic features in growing them with a view to producing new varieties. It will be enough to cite just the following fact: One group of seedlings of a number of pear combinations was receiving organic and mineral fertilizers from the beginning of its fruiting; another group of seedlings (of the same combinations) received no fertilizers. The result was that in the course of years, even in the droughty years 1946 and 1947, the seedlings which received fertilizers yielded fruits whose quality improved from year to year; their colouring also improved, and their size increased. In the case of the seedlings which received no fertilizers, these qualities in the fruits deteriorated; they are now definitely formed and do not perceptibly respond to efforts to improve them. This example shows that in the production of new varieties it is necessary to pay strict attention to the conditions of training and change them accordingly by all the means at the breeder's disposal.

The final stage in the breeding process—the propagation of new varieties—requires the greatest circumspection. The propagation of new varieties by grafting on wild stock, as is the practice of many breeders, has a pernicious effect on the qualities of the seedlings whose varietal characteristics are not yet fully formed. We propagate new varieties on seedlings of old cultivated varieties or on seedlings of the variety which is to be propagated. This eliminates the pernicious effect of the wild stock, which possesses a great power of hereditary transmission, on the qualities of new variety.

The problem of extending southern fruit cultures northward can only be solved, and is being solved by us, on the basis of I. V. Michurin's teaching. I might cite as an example the new varieties of peaches produced under conditions obtaining in Kiev.

The Institute has produced and is now propagating a peach variety, No. 981, which ripens at the end of July (this year it ripened on the 20-23 July, that is to say, simultaneously with the earliest maturing peach varieties cultivated in the extreme South of our country). The average weight of a fruit is one hundred grams. The colouring is bright red. In flavour it is no worse than the best peach varieties cultivated in the South. (Some of the comrades here present can confirm this.) The original parent of this variety perished in the severe winter of 1939/1940. The strain was grafted on various stocks. All grafts perished during the German occupation. Only a graft on a blackthorn survived the severe winters. This year it has yielded a large crop, and this is the case also with the little trees obtained by propagating the grafts on blackthorn. The use of blackthorn as stock has induced radical changes in the peach seed plant: it has become more frost-resistant and earlier maturing, while its flavour has remained good.

In the course of the production of new peach varieties one more extremely important fact has been established: the influence of the stock on the scion, which has led to pronounced changes in the scion. One peach form was grafted on an apricot. When the peach plant began fruiting the fruits were gathered and the stones planted. The seedlings, 42 in number, began to fruit this year. On six of them the fruits have been found to

be entirely destitute of pubescence which is characteristic of all peach forms.

This fact is important not only as bearing on the extent of the influence exerted by the stock on the scion and the possibility of making use of this influence to obtain entirely new forms, but, in our opinion, it helps to explain the appearance of the nectarine which is extensively grown in the United States of America.

Similar facts might be cited on the basis of the work to produce new cherry varieties. The Institute now possesses new forms of cherries not inferior to the best West-European varieties as regards flavour and other qualities, and greatly superior to them as regards frost resistance.

The division by the Mendelist-Morganists of the plant organism into hereditary substance and soma has led to the assertion that only modificational variations are possible within a clone, and that, therefore, selection within a clone is useless. But the facts show that selection within a clone is an effective method of increasing the yields also of plants propagated vegetatively.

Everyone knows, for example, that there exist in nature more than one Antonovka, more than one Papirovka, more than one Limonka, etc. However, no such multiplicity of forms existed in the not distant past; it evolved under the influence of varying conditions under which the named varieties were grown and as the result of the selection of the best plants. Selection of the best plants with the application of proper agrotechny leads to a considerable enhancement of yield. I need but point out that, as the Institute's experiments have shown, such selection in the course of three or four years in the case of currants leads to the doubling of the yield in some varieties. This example shows that successful breeding is only possible on the basis of I. V. Michurin's teachings.

One way of raising the productivity of orchards is to do away with the periodicity of fruiting. The apple and pear trees, as you know, fruit every other year. By doing away with such periodicity of fruiting we could considerably raise the productivity of our orchards. This is a problem of major importance.

Many investigators both in our country and abroad are working to solve this problem. But formalism in the way of think-

ing and, as a result, an abiological approach to the phenomenon have been the cause of the sterility of many such efforts. Following the example of the foreign investigators Kraus and Kraybill, our Mendelist-Morganists have taken the line of ascertaining the mathematical relation between carbon and nitrogen (C:N) in fruit trees in the years of their fruiting and in non-crop years, in order to make an attempt to create in the plants the relation characteristic for years of abundant crops. The investigator with such a mathematical approach to the problem loses sight of the biological characteristics of the plants and their requirements for food of definite qualities and in definite quantities.

The researches of our Institute have shown that it is impossible to do away with the periodicity of fruiting by finding the mere formula of the relation between carbon and nitrogen. The passion for mathematical exercises (like the passion for colchicine and growth-promoting substances) is an aping of foreign fashions, which often leads to serious embarrassment (as was the case with colchicine), while contributing nothing to the solution of complex biological problems of great practical importance.

Preliminary investigations conducted in the Institute show that for fruits to form buds every year a high concentration of cell sap is needed, and this is achieved by introducing fertilizer into the soil. But the high concentration of cell sap is needed only at a definite period of the tree's vegetation, namely, before the shoots finish growing, which coincides in time with the most active period of differentiation of buds. Depending on the degree of concentration of cell sap in this period, the buds become differentiated, turning either into growth buds or into fruit buds. By producing one degree or another of cell sap concentration we can regulate organ formation.

Experimental data obtained in the course of several years show that by the above-mentioned means it is possible to achieve the differentiation of all buds of fruit crops and thus find the basis for a solution of the problem of doing away with the periodicity of fruiting.

Soviet biologists, using Michurin's views and methods in their work, and further developing them, will achieve still greater success in their investigations, which will be of benefit to our Country. (*Applause.*)

Academician P. P. Lobanov. I call upon I. A. Minkevich, Doctor of Agricultural Sciences.

I. A. Minkevich. Comrades, great and serious tasks have been set before agricultural workers and agricultural science by the decisions of the February, 1947, Plenum of the Central Committee of the C.P.S.U.(B.)

The practical application of the achievements of advanced agronomical science have served as a major factor in the rapid development of agriculture in our country.

As we contemplate the questions of the further development of agricultural science and the speedy realization of new measures, we must come to the conclusion that a speedier and more thorough study of theoretical questions is now most essential. A study of these questions in regard to all aspects of agronomic science and in regard to changing the nature of plants is of extremely great importance for practice.

In regard to changing plants, it must be noted that the achievements of Soviet science have already provided clear evidence that we are in possession of perfected methods of controlling the processes of form development. Breeding work can therefore be speeded up and its effect can be heightened.

The results of the work on oil crops show that our breeding methods were in the main correct. I need but mention that 70% of the area under oil crops in the Soviet Union is sown with varieties produced by the Institute of Oil Crops. It should be pointed out, however, that if we mastered the form-building process more fully the breeding work could be speeded up and its effect heightened.

Every plant organism has formed in the process of evolution. Consequently, it is the product of long historical development which proceeded under definite conditions of the outer environment. A living organism thus represents a complex historically evolved system whose integrality presupposes the close interrelation of its separate parts. It follows from this that it is a difficult problem to change any of the properties and features of a vegetable organism. It should be noted that the more complex the structure of an organism the harder it is to produce changes in it.

All this indicates that our knowledge of the laws of the individual development of a plant organism must be based

on knowledge of the laws of the development of the species. K. A. Timiryazev says that the physiologist, in arriving at his synthesis, whether by experiment or only by logic, cannot content himself with a mere analysis of vital phenomena. He must also know the history of the organisms. The development of the individual reflects, as it were, the course of the development of the species, and in the process of individual development qualitative changes take place.

Academician T. D. Lysenko has shown that the development of an annual plant consists of several phases, two of which—vernalization and the light phase—are basic, and a definite complex of external factors is required for the plant to go through these phases. The composition of the complex will be different both for the same plant going through the different phases and for plants of different inherent properties. In determining the factors of intervention in the development of plants we must have a correct answer to the question of the genotype and the phenotype, of the role and significance of the external conditions in the development of a plant organism. Phenomena of heredity cannot therefore be reduced to the continuity only of the chromosomes of the germ cell, for alterations may develop also in other, supplementary ways. It is therefore a mistake to contrast modification and *Dauermodifikationen* to hereditary alterations. But it would be wrong to imagine that every alteration in a plant organism is hereditary. To obtain hereditary alterations it is necessary that definite changes of a biological nature should occur in the plant organism, expressed in alterations—at definite stages of the development—in the type of metabolism. And for this it is not at all essential to use powerful factors and strong doses, because in some cases they may prove biologically non-active and lead to the death of the plant or produce a monstrosity. As shown by I. V. Michurin, a disturbance in the normal processes of metabolism characteristic of a given plant organism may occur under the influence of such factors as temperature, humidity, light, etc.

I believe that our experiments with oil flax may be of some interest. Back in 1939 a dwarf variety of a single-stalk and single-boll upland flax was gathered in Uzbekistan. Its height was 13-15 centimetres. After a change of external conditions

(sowing at Krasnodar, in a zone of sufficient humidity, provision of an increased feeding area and application of basic fertilizers) in 1940-1942 a multi-stalk and multi-boll oil flax was obtained. But the most important point is that we have succeeded in perpetuating the alterations thus achieved. The oil content of the seed of the initial variety, the dwarf single-boll flax, was 42.7%, for absolutely dry seeds. After its transfer to and training in the North Caucasus (under new conditions) the oil content of this form reached 45.8%. And, as our investigations have shown, this was due not only to a reduction of the husk of the seeds, but mainly to a reshuffling and change of the components of the seed itself. A change has also occurred in the quality and quantity of the fibre. Experiments with this new form at the Odessa Institute of Selection and Genetics, at the Leninakan State Plant-Breeding Station and in other places in 1947 have shown that the oil-content character is preserved.

Another experiment (with flax) was carried out at the Don Oil Crop Experiment and Breeding Station. In 1939-1941 original seeds of the fibre flax 806/3 bred by the Flax Institute (Torzhok) were sown at the Station. Parallel to this all the subsequent generations of this variety, as reproduced by the Don Station, were sown. All the requirements of field experiment were strictly observed in the sowing. An examination of the crops and their qualities has elicited the following facts: The original seed received from the Flax Institute contained 39.3% of oil (in absolutely dry seeds); the seeds planted at the Station in the first year yielded 3.7 c. of seeds and 16.2 c. of stem per hectare. In the second year the crop per hectare was 4.3 c. of seeds and 11.7 c. of stem; the oil content was 40.1%. Analysis of the plants grown in the third year showed a yield of 4.4 c. of seeds and 7.4 c. of stem, and an oil content of 40.4%. To appreciate these results we must bear in mind that fibre flax is distinguished from the oil flax and intermediate varieties cultivated in the South by a low yield of seeds, a much higher yield of stem and a low oil content. From these facts it is to be seen that when grown under southern conditions the fibre flax shows a tendency towards a gradual increase in the yield of seeds, a lowering in the yield of stem and a higher oil content in the seeds—in other

words, it reveals a shifting, even if slow, of characters from those of fibre flax in the direction of intermediate varieties.

The process of oil formation in flax begins from the moment of flowering and continues until full maturation. The rate at which oil accumulates in the seeds under definite favourable conditions (meteorological and agrotechnical) is subject to definite laws. The factors which ensure a high crop, as a rule, also create the favourable conditions for the accumulation of oil in the seeds.

The results of our work with flax show that alterations in development can be transmitted to succeeding generations. Of course, this transmission is accomplished only as the result of repeated influences brought to bear in the course of several generations.

The facts I have cited show the correctness of the Michurin trend in biology, which proceeds from materialist positions. It means that changes in the conditions of life, in the conditions of the external environment, as a rule, inevitably lead to the breakdown of the old type of a plant organism and the creation of new forms corresponding to the new conditions of life. The Michurin trend in biology maintains that the new properties of living organisms, acquired under the influence of changes in the conditions of the environment, can be hereditary. This arms the practical worker with scientific methods for changing and perfecting plants so as to serve man's requirements, so as to breed highly-productive varieties. To deny that hereditary properties of plants undergo qualitative alterations under the influence of changes in environmental conditions, to deny that living organisms can transmit properties and qualities which arise in the living body under the influence of the conditions of life, is to take a metaphysical attitude to the matter; it means abandoning the materialist, dialectical position. One result of denying that the conditions of life of plants influence the heredity of living organisms is that selection loses touch with agrotechny, which is harmful.

We thus see that there is no break, no disconnection between organism and environment, between the genotype and the phenotype, between hereditary variation and modification. This means that we have every opportunity to take a hand in changing the nature of plants by making use of the external

conditions of their existence. This is the point of departure of I. V. Michurin's and Academician T. D. Lysenko's teachings. It is the only correct point of departure and its succinct expression is to be found in Darwin's definition of a species. By using the influence of external factors and creating suitable conditions of development, it is possible, by means of selection, to breed new, more perfect varieties of plants.

This premise is borne out by the facts of practical breeding and by the history of the development of cultivated plants. By making use of the external conditions and directing them in such a way as to best bring out the properties and organs for the sake of which the given plant is cultivated, we can gradually alter and improve the plant along the lines we need, perpetuating the useful alterations by selection. The sunflower may serve as an example of an exceptionally rapid process of form development under the influence of the above-mentioned factors. It is relatively recently, as we know, that the sunflower became a cultivated plant; yet in this short space of time there has evolved a complete species of cultivated sunflower with numerous ecotypes.

The degree to which various forms and strains of oil plants react to external conditions varies. Forms and varieties with sharp fluctuations of economical and biological properties depending on environmental factors cannot be expected to range over a large area. They can only be cultivated in a limited number of regions where the soil and climatic conditions are conducive to the development of the positive properties.

The factual material concerning oil plants shows that vegetative hybridization opens up new ways of directing the form-building process and should in future be widely used in breeding new varieties of oil plants. To make sure of success in this matter, it is necessary, as in the case of sexual hybridization to create training conditions conducive to the development of the properties and characteristics which it is desired to obtain and to fix in the progeny. It has been established, on the basis of the Institute's work in recent years, that a highly promising method for obtaining new initial material for sunflower breeding is the free intervarietal cross-pollination of varieties distinguished by high yield, high oil content, resistance to broom rape and other positive properties, but of different

origin and cultivated in the course of several generations under different conditions and in various places. The materials and facts accumulated by the Institute in recent years attest that the larger tasks in regard to the production of oilseeds can no longer be tackled by the old methods. At the same time the new methods must be devised in conformity with the advanced Soviet teaching in biology, which opens up boundless vistas in this sphere. (*Applause.*)

Academician P. P. Lobanov. The Presidium has received a note with a question which I consider it necessary to make public: "Why has no one of the adherents of formal genetics taken the floor? Is it because they do not want to speak themselves, or because they are not being given a chance to speak?" I answer: None of them has asked for the floor, but it is to be presumed that they will take the opportunity offered them to speak at this session. It would be incomprehensible and unworthy of the status of scientists if they kept silence while fundamental questions of science are discussed.

Permit me to adjourn until 6 p. m.

THIRD SITTING

Evening, August 2, 1948

Academician P. P. Lobanov. We shall continue the proceeding of our session. I call upon Professor N. I. Noujdin.

Professor N. I. Noujdin (Institute of Genetics of the Academy of Sciences of the U.S.S.R.). The controversy in the field of biology has been going on for twenty years, and attention has been concentrated mainly on the problems of heredity and variation, and also on the problems of evolution.

In the beginning of the 'thirties a struggle against Menshevist idealism flared up in the field of philosophy. This struggle was not confined to philosophical problems; it touched other branches of science, and biology in particular. In the latter field, the struggle centred mainly around genetics, for it was here that Menshevist idealism found most striking manifestation.

If we recall the problems around which the struggle raged, it will be easily seen that there is a direct connection between the struggle against Menshevist idealism and the controversy that has flared up in connection with the work of Academician T. D. Lysenko; and the latter phase is the logical continuation of the struggle that was launched against Menshevist idealism.

It must be emphasized that in that period a number of fundamental errors were noted in the field of genetics, among them being Weismannism, autogenesis, and the underration of the role of environment. At that time geneticists did not deny the justice of the criticism levelled against them and they promised to rectify their errors in their subsequent work.

In 1932, at a conference held to plan genetic science, A. S. Serebrovsky stated in the program report that he made: "It must be said that we Soviet geneticists and breeders are still to a large degree vehicles of bourgeois science. . . . Our science must be radically reconstructed in order that it may deserve to be called Soviet science, the science of socialist society."

This absolutely clear, although unflattering, admission inevitably had to lead to the corresponding conclusion that it was necessary radically to reconstruct the whole of the work in the field of genetics, to revise a number of the theoretical propositions of the science of genetics.

These conclusions were drawn, but unfortunately no reconstruction, no critical revision of the bourgeois science to which Professor Serebrovsky referred, took place. Naturally, therefore, the controversy in the field of genetics flared up with renewed vigour.

In addition to the old errors, new ones were revealed; new problems and tasks in the field of breeding and genetics in our country were brought to the front; new paths which our genetics should pursue were indicated.

The experience of the past 15 years points to a very important circumstance. The representatives of formal genetics have not made a single serious attempt to reorganize their work, to criticize the fallacious propositions of formal genetics in the sphere of the theory of heredity that were pointed out to them.

The question arises: were they unwilling to bring about this critical reconstruction, notwithstanding the repeated warnings they had received, or were they incapable of doing so? It seems to me that the former is the case—they were unwilling to reorganize their work. A number of examples can be quoted to illustrate this. It is sufficient to point to Professor Zhebrak's article in the magazine *Science,* from which it is evident that the formal geneticists are ready to collaborate in a united front with the most reactionary bourgeois science of genetics.

In that very same magazine *Science*, N. P. Dubinin, while speaking of the achievements of Soviet genetics, said not a single word about a whole trend in our science—Michurin genetics. This omission was due to the desire to emphasize to all bourgeois geneticists that there is a definite group in the Soviet

Union that does not regard Michurin genetics as a scientific trend.

Take, for example, the attempt that was made to organize in the Academy of Sciences of the U.S.S.R. a second Institute of Genetics that would represent a trend in opposition to that which the Institute of Genetics of the Academy of Sciences of the U.S.S.R., directed by T. D. Lysenko, is at present developing.

All this shows that the matter here is not failure to understand, but unwillingness to reorganize, in the hope that it will all blow over, as it blew over in the past. More, there is a striving here to push into the forward positions in the science of biology.

We are in the habit of saying that a discussion is going on in the field of genetics. Actually, there is no scientific discussion; the discussion ended after the conference that was held at the editorial offices of the magazine *Pod Znamenem Marxisma*. Since then we have not had a discussion, but a very unseemly struggle, waged by the representatives of formal genetics against the progressive Michurin theory. There is no need to give examples of this struggle here; the fact remains that there is now no constructive scientific discussion; what we have are coteries, and a conflict which is assuming the most abnormal, unseemly forms. This must be stopped quickly, because this conflict is hindering our work, is hindering us in training cadres, retarding the development of genetics and selection, and consequently, is causing immense harm to theory and practice.

One cannot help asking oneself: how is it to be explained that the discussion in the field of concrete problems developed into the open struggle which the representatives of formal genetics are waging? The explanation is that the representatives of the formal genetics trend have proved incapable of refuting by experiment the fundamental propositions that were presented in the course of the discussion by the Michurin trend in genetics. It is one thing to come out with general declarations, but to come out with factual data on the problems raised in the course of a discussion is quite another. It is no accident that throughout the whole period of the controversy the representatives of formal genetics did not complete a single experiment on the controversial problems that were raised in the

course of the discussion. This is one of the causes of the situation that now exists in the field of genetics.

Another cause is the successful development of Michurin genetics. In contrast to the group of formal geneticists, the representatives of the Michurin trend have, during this period, accumulated a large amount of experimental data which cannot be denied.

Let us take as an example the summer planting of potatoes. This is not merely a practical measure. It is based on the profound theoretical doctrine of variation in the nature of organisms in accordance with the conditions under which they are reared. The conclusions drawn from the work of summer planting of potatoes can be widely applied in the science of biology. Hence, the heated debates about the nature of the degeneration of potatoes, and the attempts that were made at the opening of the discussion to reduce the question of the nature of the degeneration of potatoes to one of virus diseases, were not accidental.

Permit me to read the following passage. "The Institute [this refers to the Odessa Institute of Selection and Genetics] should seriously take up the study of virus diseases . . . until it does, a useful measure, now widely practised, will remain without a scientific explanation." This was written some time ago by P. N. Konstantinov, P. I. Lisitsyn and D. Kostov.

All the subsequent work proved that, at that time, summer planting had been given correct, profoundly theoretical substantiation. The summer planting of potatoes enables us to draw broad conclusions in the sphere of theory.

Let us take another example—the elective fertilization of plants. Comrade Olshansky, in the speech he delivered here, has already given you numerous examples of what has been done in this field of work. The problem of elective fertilization that was raised by Academician T. D. Lysenko in the course of the discussion was not only of important practical significance as one of the methods of regenerating strains (intravarietal crossing), but also of great theoretical significance for the problems of genetics and the theory of evolution. Suffice it to say that if fertilization is not accidental, the per-family analysis of the hybrid offspring is of exceptional significance in plant breeding, for the results of segregation in F_2 will vary.

Hence, all the summarized Mendelian conclusions often quoted by the formal geneticists in various manuals on selection lose their significance.

The presentation of the problem of elective fertilizers met with the sharp hostility of the formal-genetics trend. "The concept that the egg of the maternal plant chooses the best spermatozoid out of all the possible pollen grains, with their different genetical compositions, that can take part in fertilization, is not confirmed by practice. The success of fertilization depends upon the rapidity of growth of the pollen tube, and upon a number of other factors." This was written by P. N. Konstantinov, P. I. Lisitsyn and D. Kostov.

B. Vakar wrote: "Starting off with Darwin, Academician Lysenko, with his theory of intra-lineal crossings, is obviously slipping into anti-Darwinist positions."

Experience since that was written has proved who was right.

At the present time we have the splendid summary drawn up by Comrade Babajanyan, in which he describes hundreds of experiments in which elective fertilization is clearly demonstrated: it is proved that fertilization does not take place by chance.

Thus, on this question, too, it is no longer possible to come out with general phrases, as was the case before.

Lastly, let us take another problem, that of vegetative hybridization. There is scarcely another question among the controversial questions that has roused such strong objection on the part of the formal geneticists. Even to this day attempts are made to deny the fact that inherited variations due to grafting are stable. Nevertheless, the formal geneticists have not conducted a single experiment that has proved the unsoundness of the claim that it is possible to obtain hereditary variations with vegetative hybridization.

The vast mass of material which the Michurinists have collected leaves no doubt that grafting is an extremely potent and interesting factor in altering heredity. This method is now being employed not only in solving theoretical problems of genetics, but also in the solution of practical problems.

It will be interesting to compare two types of declarations on this subject. In 1936, Professor A. R. Zhebrak wrote: "... we do not think that transplantation can lead to specific changes

that could serve as a basis for practical plant breeding, because nobody has demonstrated the specific action on the genotype of the scion, and all speculation on this subject is aimless."

Academician S. S. Kanash, in summing up his work on plant breeding and seed cultivation of cotton, writes: "We employ the methods of intraspecific, interspecific and vegetative hybridization. . . . We also employ vegetative approximation as a method of controlling the nature of plants which enables us to promote all the processes of development."

In an article by Academician Zhdanov we read: "The factual material available testifies to the fact that vegetative hybridization opens new ways for directing the form-building processes and should receive wide application in the cultivation of new varieties of oil-bearing crops."

How far removed are these utterances of people who are directly engaged in creating varieties from what A. R. Zhebrak once wrote.

I will limit myself to these three examples, although very many more could be quoted. They prove that although it may have been possible in the beginning of the discussion to argue in a general and, at times, derisive way, the situation now has sharply changed.

The development of the Michurin theory, and also the correctly indicated path of struggle—the experimental solution of controversial problems—have been one of the causes that led the formal geneticists to abandon the method of debate and adopt the methods of conflict. The Michurin trend points to the path of progress of the entire science of genetics.

The formal geneticists themselves are collecting increasing numbers of facts which can no longer, without grave overstretching, be fitted into their own scholastic constructions which are quoted in textbooks on genetics as "indefeasible" truths. These theories include the theory of the gene, variation, Mendelism and so forth.

In this connection I want to dwell on two problems, namely, the problem of variation and the problem of the gene. There is no doubt that the line of principle that divides the formal geneticists from the Michurinists is the interpretation of the nature of inherited variations. The Michurinists take as their point of departure the unity of the external and the internal. Only on

the basis of the contradictory unity of the external and internal in variations, does the external, passing into the internal, become the basis of development. Taking this as their point of departure, the Michurinian geneticists, setting out to direct variation, proceed by the road of directing the process of development, and not by the road of seeking specifically acting mutagenic factors. According to the opposite side, the development of the organic world does not proceed on the basis of the unity of the external and the internal. That side always contrasts the external to the internal; it holds that there is only a mechanical connection, but no dialectical unity, between the external and the internal. It regards environment only as a factor capable of accelerating the mutation process, which, by virtue of its intrinsic causes, proceeds quite normally, uninfluenced by environment.

Up to 1927, the crudest autogenetic conceptions of variation held unlimited sway in the field of genetics. It was believed that the gene could not be changed by any external action. Some geneticists asserted that it was possible to burn, to poison the gene, but impossible to change it.

The work of the late Filippov, and that of Muller in 1927, proved that by means of induced action it was possible to change the gene, to obtain hereditary changes, or mutations.

The significance of the work of these two men lay in that it removed the crudeness of the autogenetic theory, but it did not abolish the theory itself. The geneticists regarded X-ray mutations as the prototype of all hereditary variations and drew the conclusion that external conditions do not cause hereditary variations, but merely accelerate the process of mutation.

In 1929 Dubinin wrote: "The influence of these actions is totally unspecific, and the most diverse hereditary changes are obtained. The process of mutation is accelerated, but all the features of the normal process are preserved." He said the same thing in 1937: "It turned out to be very difficult to alter the general speed of the process of mutation, and it was not until 1927 that Muller, utilizing X-rays, showed that external factors are capable of accelerating the process of mutation."

As you see, in all cases it is argued that even a powerful factor like X-rays is incapable of producing a change in heredity.

All that is admitted is that the gradual process of mutation is accelerated.

Recent researches, however, have revealed the utter absurdity of the assertion that the environment does not cause specific changes.

I have dwelt especially on environmental factors that belong to the category of abiological factors.

Recent researches have shown that even X-rays exercise a definitely specific influence upon the process of variation. The hereditary changes brought about by the influence of powerful, and certainly not specific, environmental factors like X-rays, ultraviolet rays, and various chemicals, nevertheless, are specific. The specific nature of the process of mutation increases many times under natural conditions owing to the vast variety of influences that affect the organism at the most diverse steps and stages of its life.

The specific nature of the influence of the above-enumerated factors is so definite that the formal geneticists are beginning to regard them as the mastering of the process of directed variation. Thus, N. I. Shapiro writes: "Summing up the branch of work involving the directed production of a definite type of mutation, we have the pleasure of noting considerable success along this line. We have already discovered a number of important features of the mechanism of the appearance of mutations, and on the basis of our knowledge of these features we have worked out effective methods of obtaining a directed, definite type of mutation." This refers not to the attainment of hereditary changes in rearing the organisms under definite environmental conditions, but to the action of factors like X-rays and ultraviolet rays. Consequently, even on the basis of such data it is no longer possible to say that influence is unspecific. The possibility of obtaining directed variations and controlled changes cannot be denied even when abiological factors like X-rays and ultraviolet rays influence the organism. But work in the field of variation has gone further. Everybody is familiar with the results obtained by Avery. His work has been so clean in genetic respects that nobody has come forward to challenge his researches.

In 1918-1924 Guyer and Smith conducted their experiments, which later caused a sensation, in hereditary changes in defects

of the crystalline lens of the eye. They took the crystalline lens of a rabbit, ground it up in a physiological solution and immunized a hen with it. Then they took some blood of the hen and injected it into a pregnant rabbit. In the end they obtained offspring with a defective crystalline lens. These experiments are quoted in all textbooks as an example of fallacious and unsound Lamarckian experiments. Recently these experiments were repeated by the American researcher Hyde, and the results obtained by Guyer and Smith were fully confirmed. This was reported by the well-known geneticist Sturtevant.

Comrade Belenky has already spoken here of the results he obtained in experiments on the same lines as those made by Brown-Séquard. You will no longer find unfavourable comments on these experiments in the literature on genetics. The experiments of Brown-Séquard, which caused as much sensation as those of Guyer and Smith, are now accepted in genetics as scientifically substantiated and quite possible. Hence, the issue is not simply directed variations, but directed variations corresponding to the influences. Permit me in this connection to quote another passage from the work of Shapiro: "The recognition of the possibility, in individual cases, of corresponding variations of genes and characters does not contradict any of the achievements of the modern theory of heredity, and primarily, of the chromosome theory."

During the discussion at the editorial office of *Pod Znamenem Marxisma,* however, Professor Dubinin said the following about corresponding effect: "I deem it necessary to say here that we are of the opinion that the path that Academician Lysenko has taken—that of obtaining corresponding directed variations by re-training plants—is a wrong one, we think it is mistaken." Corresponding variability was the bogey that the geneticists wanted to play on. How sharply the situation has changed in the field of genetics itself—formal genetics no longer provides our formal geneticists with a basis for continuing the scientific discussion. Take the problem of the gene. I will not dwell on the critical part of the problem. Everybody knows that this is one of the most metaphysical spheres of genetics. But at present all is not well with the problem of the gene in the field of genetics itself. During the past decade distinguished geneticists like Goldschmidt have levelled criticism on the problem of the

gene. For example, he said that a number of observations show that there are no genetic mutations, hence, there are no genes and that "a theory of the germ-plasm would have to do away completely with the concept of genes as units."[1] Goldschmidt, the author of the "quantitative theory of the gene," is compelled by the weight of factual data to throw doubt on the gene. In criticizing the gene theory, Goldschmidt commits numerous errors—that is understandable, for he levels his criticism from the crudest mechanistic standpoint.

How did our geneticists react to the criticism of the gene levelled by Goldschmidt? Instead of utilizing the rational grain that his criticism contains and bringing forward their own proper criticism of the gene, and of Goldschmidt, our geneticists began to defend the gene against the attacks of Goldschmidt.

In 1947, Comrade Alikhanian wrote in his work: "Repudiation of the genes led Goldschmidt to repudiate the role of hereditary variation in evolution. He entirely repudiates Darwin's principle of gradual development. . . ."

Comrade Alikhanian tries to frighten us with the prospect that repudiation of the gene necessarily creates the danger of slipping into anti-Darwinism.

As if in anticipation of criticism like that levelled by Alikhanian, Goldschmidt wrote that he did not take the stand of anti-Darwinism because he repudiated the gene; he had repudiated Darwinism as far back as the 'twenties, i. e., when he adhered to the classical standpoint of the theory of the gene.

The two examples taken from the sphere of variation and the problem of the gene show that the geneticists themselves are compelled by the weight of evidence more and more to take the standpoint of Michurin genetics, without, it is true, formulating it as Michurin genetics. They try to give all these phenomena a different explanation, but the objective facts of nature, which they cannot get away from, show that the Michurin path is the only correct path. "It will also be seen, however, that the views of geneticists all over the world are gradually beginning to change, so that many of the principles which a few years ago were regarded as immutable laws are now seen to be at

[1] R. Goldschmidt, *Physiological Genetics*, New York and London 1938, p. 311.

the best either mere approximations or undue generalizations."[1] This passage is not quoted from the work of a Michurin geneticist; it was written in 1945 by Hudson, who correctly evaluated the situation in the field of formal genetics itself.

Herein lies the reason why, instead of conducting a scientific discussion, the formal geneticists have taken the stand of coteries, the stand of open struggle. The sooner this is stopped, the sooner the geneticists adopt the Michurin standpoint, the more successfully will genuine scientific genetics develop, and the sooner will we scientists carry out the tasks that the Party, the Government and Comrade Stalin personally, have set us. (*Applause.*)

Academician P. P. Lobanov. I call upon Professor N. M. Sisakian.

Professor N. M. Sisakian (Corresponding Member of the Academy of Sciences of the Armenian S.S.R.). Comrades. In his address, Trofim Denisovich Lysenko gave us a profound analysis of the present state of biology. The principles that T. D. Lysenko developed in his address are directly related not only to biology, but to the other branches of natural science. The Michurin ideas that T. D. Lysenko expounded in his address are correct, progressive. They are near and akin to us Soviet biochemists, the pupils of that distinguished representative of Soviet science, Academician A. N. Bach.

Of particular value for us, the representatives of the Soviet biochemical school, are T. D. Lysenko's references to the close connection that exists between variation, heredity and the metabolic processes (the character and type of metabolism in the organism).

The merit of the Bach and Oparin school consists in that enzymes in the works of this school, for the first time became a powerful instrument for the study of metabolism and for directing the enzymatic processes in working on materials of vegetable or animal origin. Before the work of Bach, Oparin, and their pupils, the action of enzymes was studied in their solutions, artificially produced from destroyed plant or animal

[1] P. S. Hudson, "Plant Breeders and Genetics To-day," *The Advancement of Science*, 1945, No. 11, p. 266.

tissues. The data obtained in this way is, of course, valuable for the purpose of ascertaining the chemical nature of enzymes, of studying the kinetics of enzymatic action, and also of investigating the activity of enzymes in autolytic mixtures.

But these data can give us no idea of the work of enzymes in the living cell, in which the conditions are far more complex than in autolytic mixtures, in destroyed and dead tissues. Enzymes are of great interest for the biologist not only in themselves, but also, as Alexei Nikolayevich Bach expressed it figuratively, as the key to our knowledge of the chemistry of living phenomena.

Referring to the direct causes that give rise to the fluctuations of enzymatic activity in the animal organism, A. N. Bach wrote twenty-five years ago that variations in the action of enzymes can be explained only by the inconstancy in the concentration of enzymes, or by the variation in their activity at certain moments due to the influence of various conditions.

Taking the views of Michurin and Lysenko as our point of departure, and basing our work on the principles of A. N. Bach's school of biochemistry, we in the Bach Institute of Biochemistry of the Academy of Sciences of the U.S.S.R. undertook researches which led to the discovery of a number of new facts that testify to profound changes in the biochemical activity of organisms under the influence of vernalization and vegetative hybridization.

Already in 1936, we were able to establish that in the vernalization of seeds according to T. D. Lysenko's method, fundamental changes of a biochemical character take place in vegetating plants.

The aim we set ourselves in this series of experiments was to trace the changes in the process of enzymatic formation and breakdown of sucrose in the living cell that occur under the influence of vernalization.

Experiments with the winter wheat Ukrainka showed that in non-vernalized plants, the process of enzymatic formation of sucrose predominates over the hydrolysis process, the breakdown of this substance. In vernalized plants, however, we see the very opposite. The process of vernalization conditions the shifting of the enzymatic equilibrium in the living leaves of plants in the direction of hydrolysis.

It must be observed that high productivity in plants is connected with the predominance of hydrolytic reactions in its vegetating organs.

It must be stated that with vernalization, not only does the direction of the enzymatic formation of sucrose change, but so also does the balance of the dissolved sugars. Vernalization leads to an increase in the quantity of monosaccharoids.

To obtain a complete and true judgment of the results arrived at, we conducted experiments similar to those just described on different varieties of cotton, a plant that differs from wheat both in its nature and in the factors required for its vernalization. The results of our experiments on cotton fully confirmed the data we obtained in our experiments on wheat. In cotton, as in wheat, vernalization conditions a fundamental change in the direction of the processes of enzymatic formation and breakdown of sucrose.

Our investigation of the action of enzymes in the living plant cell under the influence of vernalization led us to the following main conclusions.

The vernalization of seeds fundamentally changes the correlation between the enzymatic synthesis and hydrolysis of the substances in plants. In the leaves of vernalized plants the process of enzymatic breakdown of substances rises sharply.

As a result of this, during vernalization, the relation between the synthesis and hydrolysis of sucrose is reduced. It must be observed that in their natural state the early ripening of these plants is due to the predominance of the hydrolytic properties of the enzyme.

It must be pointed out that the correlation between synthesis and hydrolysis in plant tissues is a rather characteristic species quality, although it changes regularly in the life cycle of a plant. But the changes in the biochemical activity of plants due to vernalization are of a character entirely different from those that we obtain, for example, by the use of ether, or by dehydrating, wetting, etc., the living tissues.

In such cases we obtain a change of a local character. These changes bear a reversible character, and after these influences are removed they disappear without leaving a trace in the subsequent life of the organism. During vernalization, however, the changes that take place in the biochemical activity of a

plant are of a non-reversible character and are preserved in the subsequent course of development of the organism.

For the sake of greater clarity I would like to deal here with another important fact. Comparing winter forms with spring forms of wheat, Academician A. I. Oparin was able to show that winter wheat is always characterized by a higher relation between enzymatic synthesis and hydrolysis, i. e., a relative predominance of the synthetic direction of the reaction over the hydrolytic. In the case of spring wheat, the relation is shifted towards hydrolysis. With the vernalization of the seeds of winter plants, the correlation between synthesis and hydrolysis within them changes and approximates to the type that is usual for spring plants, and, as was shown, the change in the enzymatic equilibrium brought about by vernalization is preserved in the plants until vegetation is finished.

Attaching enormous importance to the influence environment exercises upon the course of development of a hybrid seedling and upon the quality of the strain that is being reared, I. V. Michurin, as is known, regarded the mentor method as the most powerful means of influencing phasically young plants.

The work I conducted in conjunction with B. A. Rubin with the object of ascertaining the biochemical activity of organisms engendered by vegetative hybridization, led to the establishment of certain definite rules.

We endeavoured to ascertain whether the activity of the enzymes of a phasically young scion is affected by its grafting on to the crown of a phasically old tree, i. e., whether this activity is connected with the nature of the mentor. To study this question we, with the kind consent of S. I. Isayev, utilized the results of the experiments that had been conducted by the Plant-Breeding Department of the Michurin Central Research Institute in the town of Michurinsk.

These researches showed that in the majority of cases grafting on to the crown of the mentor causes sharp changes in the activity of the oxidizing enzymes of the grafted phasically young organism, and the direction of these changes are determined by the nature of the mentor. The late varieties of apple trees that were used as mentors, as a rule, caused a higher activity of the peroxidase of the seedling, whereas its grafting on to the crown of an early variety usually led to a lowering of this

activity. For example, in the Grushovka/Bellefleur-Kitaika hybrid combination, judging by the peroxidase indices, part of the seedlings turn in the direction of the Bellefleur-Kitaika and part in the direction of the Grushovka. We observed similar changes also in other hybrid combinations.

We saw a similar picture in another series of experiments in which the buds of hybrid seedlings were grafted on to the crowns of mentors. We showed that the late varieties have a more acute peroxidase and a less active invertase. It turned out that the influence of the properties of the mentor causes a reconstruction of the enzymatic system of the buds. The buds of the late varieties show a more active peroxidase and a less active invertase.

Thus, these data fully bear out what I. V. Michurin said about the interrelation between phasically young and phasically old organisms. In causing late-ripening in the seedlings we at the same time increase its enzymatic activity, and vice versa.

Naturally, these interrelations between biochemical characters can exist only if they physiologically condition each other.

That the changes we have revealed are not fortuitous but conform to a regular law and are conditioned by the complex interrelations that arise in hybridization, is proved by the results obtained in the work I conducted in conjunction with I. E. Glushchenko.

We conducted a comparative biochemical investigation of the seed generations of vegetative and sexual hybrids of tomatoes.

Experiments conducted with the seed generation of the vegetative hybrid combinations: Humbert/Golden Queen, Ficarazzi/Golden Queen, Mexican 353/Golden and Planovy/Yellow Pear form, clearly showed that the morphological changes that take place in the seed generation of vegetative hybrids definitely affect the biochemistry of the fruit as well as the biochemical activity of the plant's assimilating apparatus. Often, as a result of vegetative hybridization, new qualities appear that were absent in the initial pair, and useful characters are strengthened. For example, the leaves of F_4 of the seed generation of the hybrid combination Humbert/Golden Queen acquire a high capability for the enzymatic synthesis of sucrose, which capability was entirely lacking in the initial forms. In another case, the fruit of the seed generation (F_1) of the hybrid combination Humbert/

Ficarazzi contain twice the quantity of vitamin C contained in the parental pairs.

Thus, grafting not only mobilizes the potential possibilities of the hybrid pairs, but, and this is particularly important, creates new qualities, it changes the character and type of metabolism.

As a rule, judging by the biochemical activity of the organism, the seed generation of vegetative hybrids displays characters of the component in the direction of which the morphological changes take place.

The F_2 and F_4 seed generation of the combination Ficarazzi/Golden Queen revealed a resemblance to Golden Queen on the following points: the amount and structure of sugars, the activity of the peroxidase, and the carotinoid content. As regards quantity of ascorbic acid, general acidity and activity of polyphenolase, they reveal the influence of Ficarazzi.

The fruit of the seed generation of the combination Mexican/Golden show two points that are characteristic of the stock: activity of peroxidase and content of ascorbic acid. The other characters, namely, composition of sugars, general acidity, quantity of carotinoids and activity of polyphenoloxidase, reveal the influence of the scion.

Thus, the characters of the generation depend not only on the characters inherent in each of the parents, but also on a definite combination of these characters.

With vegetative hybridization we very often get plants which produce fruits that differ not only in shape, but also in colour. This feature is particularly marked in seed generation. We have investigated the composition of different fruits, the colour of which differed in the same cluster, and we found that the fruits that were varied in colour also differed considerably in chemical composition.

In another series of experiments we examined the biochemical indices of the seed generation of vegetative and sexual hybrids of Golden Queen and Sparks, and Humbert and Ficarazzi.

It was revealed, for example, that in F_3 of the vegetative hybrids of Golden Queen/Sparks, activity of peroxidase in the yellow fruit is expressed by the magnitude 16.1, i. e., considerably in excess of the activity of the enzyme in Golden Queen, and on this point approximate to the Sparks type.

In the yellow fruit of F_3 of the sexual hybrid of Golden Queen × Sparks, the activity of peroxidase is equal to 13.6. As you see, the similarity is very close.

The character of the changes in general acidity is approximately the same. Thus, in both vegetative and sexual hybridization the changes in the activity of oxidizing enzymes in yellow fruits shifts sharply in the direction of the Sparks type.

Consequently, in the fruits of the seed generation of both vegetative and sexual hybrids, changes in biochemical characters conform to law and are identical.

Comrades. Workers in the field of Soviet biology have every reason to be proud of the magnificent galaxy of scientists of the great Lenin-Stalin epoch. Timiryazev, Pavlov, Michurin, Williams and Bach were giants in the realm of thought who, each in his own sphere, revolutionized science.

For us biochemists the value of the heritage of the great scientist Michurin, the effectiveness and transformative character of Michurin's theory, which has been further developed in the works of T. D. Lysenko, are determined not only by the magnificent quality of the varieties they have created, not only by the distinguished contribution they have made to biological science, but also by the fact that the Michurin theory has opened new vistas, has created wide opportunities also for biochemical research. (*Applause.*)

Academician P. P. Lobanov. I call upon Professor S. G. Petrov.

Professor S. G. Petrov (Scientific Research Institute for the Poultry Industry). An extremely important item in the work of Michurin was the extensive utilization of initial breeding material. To obtain new strains he used not only local strains that were adapted to the given concrete environmental conditions, but also foreign, overseas, alien strains. The celebrated zootechnician M. F. Ivanov, pursued the same path in his day. In 1925, he imported fine fleece sheep from abroad for the purpose of crossing them with the low productivity Askania flock. On the basis of the crossbreeds that he obtained he produced the splendid Askania merinos.

After M. F. Ivanov, this method was employed by sheep breeders on an extremely wide scale. Over 100,000 merino

sheep were imported from abroad and utilized for crossbreeding. Today, the majority of the sheep in this country have "changed their wool." Formerly, the wool was coarse and unsuited for weaving fine cloth; today, it is soft and good. The U.S.S.R. has been freed from the necessity of importing merino wool.

This method of utilizing the best breeds available in the world was also adopted by our poultry breeders. This is the group of zootechnicians that I am connected with. I collaborate with them in the capacity of geneticist and breeder. Why did we take the line of utilizing foreign breeds? For a very simple reason. Before the October Revolution there were no large poultry farms in Russia. There were only poultry yards connected with ordinary farms. When sovkhozes and kolkhozes, with their own poultry farms, began to be formed, it was found to be very difficult to raise the laying capacity of the native stock above 50-60 eggs per annum. For example, the wealth of experience gained by a sovkhoz like the Borka farm, near Kharkov, which before the war had 50,000 laying hens, showed that it was a waste of time to attempt to do selection work with common native stock.

On the initiative of V. A. Mikhalkov, Leghorns were imported into the North Caucasus. The question then arose as to whether it was worth while bothering with this tender and delicate breed, which stood in danger of perishing in the farms we had then. To this question Mikhalkov wittily replied: "We can learn to handle highly productive breeds only if we have them; if we have no highly productive breeds we shall never master high techniques of stockbreeding, and of poultry breeding in particular."

After several years of acclimatization of Leghorns in our kolkhozes, Zaitsevsky, the colleague and pupil of Mikhalkov and continuator of his work, collected the best birds in the breeding station in Pyatigorsk and there successfully engaged not only in their propagation, but also in the improvement of the breed.

Before the war the Pyatigorsk Breeding Station was the best poultry farm in the U.S.S.R. The average egg production of the hens there was 212 eggs per annum. Hatching eggs from this farm were sent to all parts of the Soviet Union. From

the Leningrad Region, Byelorussia, Central Asia, from everywhere, came the demand for Pyatigorsk Leghorns, not for American Leghorns, but for Leghorns adapted to our conditions.

A number of sovkhozes followed the example of the Pyatigorsk Breeding Station. I can mention several of them, prize winners at the All-Union Agricultural Exhibition. At the top of the list is the Krasny Sovkhoz in the Crimea; it had 100,000 laying hens, and every year sent 14,000,000 eggs to Moscow and other places. Selection work was conducted at this farm on exemplary lines. Other farms in this list are the Udarnoye Sovkhoz in the Voronezh Region, and the sovkhozes near Moscow and other places. The breeders at these farms improved the foreign Leghorns by crossing them with local breeds.

In 1941, just before the war, these farms had what were called "long lived" birds; they were called so because they laid for a period of 5-6 years at an invariably high level. Many of these birds laid more than a thousand eggs in the course of their lives.

It is edifying to note that a number of these farms produced new, original varieties of birds. The Krasny Sovkhoz produced a group of birds without large wing feathers; these birds cannot fly. This is exactly the type of bird that our country needs. The egg production of these birds was not inferior to that of the record holder hens.

The same thing happened to the Leghorns—the best egg-laying breed in the world—as happened to many other foreign breeds—they changed. A few years ago we were still being accused of having a predilection for things foreign; but our work in 1945, 1946 and subsequent years has shown that this is wrong, that the accusation is groundless.

In 1945, the first peace cargo from America consisted of hatching eggs carried by air across Siberia. When we hatched these eggs and compared the chicks with our Leghorns, we found that they differed sharply from them. The American Leghorns are extremely capricious and delicate. Our Leghorns are not by any means exacting. This shows that in a matter of 10-12 years we transformed the American Leghorns.

The war inflicted severe damage upon our poultry industry. Many breeding farms were destroyed. The pedigree stock of the Pyatigorsk farm in particular was reduced to 500 laying hens. Not a single cock was left. Later I learned why this happened.

This farm was handed over to an S.S. man who regarded it as his private estate. When the Soviet Army drew near to the farm the S.S. man could think of nothing better to do than burn all the selection records and slaughter all the cocks with his own hand. Only hens were left, and these highly productive layers had to be crossed with Leghorn cocks obtained from neighbouring kolkhoz poultry yards.

The effects of the war upon our poultry breeding industry are rapidly being wiped out. In 1948 the farms near Moscow showed incubator figures never before attained anywhere in the world. Our incubator operators get 85 chicks from every 100 eggs, whereas in the United States the percentage never exceeds 70. In Pyatigorsk we already have hens that lay 200 eggs per annum.

We are now developing a new form of poultry farming—breeding for the meat. Before the war we concentrated mainly on egg production; now we want to breed poultry for the meat, and we want the meat to be of the most delicious kind. I may say that in a few months' time the people of Moscow will be able to taste, only a little perhaps, but still to taste the delicious meat of especially bred, heavy, meat chickens. And in a year or two they will be able to have fresh poultry both in the winter and summer.

By what methods have we Soviet poultry breeders been working? Our activities reflect the entire history of genetics. In the 'thirties we were held in the bonds of the genetic theories that predominated at that time. None other than Mikhalkov issued a work of such a formalistic nature that it was condemned even at that time. Professor A. S. Serebrovsky carried out an enormous amount of organizational work in the endeavour to turn poultry breeding to the lines of repeated inbreeding. But this too was a failure, although we did practice inbreeding for the purpose of "rounding off" the best families.

A number of formalistic passages may also be found in my book, published, I think, in 1934. It was the fashion then.

But mastery of the dialectical method, thoughtful study of the works of zootechnicians, genuine zootechnicians, and the discussions that were held not only within these walls, but also in the press, soon told upon our work and upon our theoretical approach. We worthily appraised the instructions of M. F. Ivanov concerning the enormous influence environment, and

feeding in particular, exercises upon the production of breeds. It is not easy, of course, for a man who has been trained in formal genetics, and who has been busy drawing the chromosome chart of a hen, to understand the close and inseverable connection that exists between the organism and external environment, the connection between the breed and the environment in which it is living and being produced.

At one farm we worked out a peculiar method of family-group selection. In the selection of poultry it was regarded as essential to cross every female with one definite male. But this method is useless for industrial breeding, because here the female never pairs with one single male, she always pairs with scores and hundreds of males. We have succeeded in working out a method of family-group selection, which has been applied in practice, and today individual crossing is practically a thing of the past.

M. V. Orlov, of the Zagorsk Poultry Breeding Institute, is conducting an interesting experiment. He is striving to increase the productivity of poultry by bringing external factors to bear upon embryonic development. At a definite stage of development of the embryo the external environment is changed, and as a result of this the organism is changed.

I would like to deal with a very interesting question that Academician N. G. Belenky touched upon here, namely, courses of lectures and textbooks. In 1938 I gave up teaching when I went in for my Doctor's degree, but last year I was given the opportunity to resume this work.

While lecturing on the subject of genetics I realized two things. First, the textbooks now available are, of course, out of date. It was very difficult to recommend them to students. I was obliged to have long talks with many students and explain to them how some of the things in the textbooks were to be interpreted. Many of the pages of the textbooks had simply to be crossed out. Those books were written 10 or 12 years ago when we were still "youngsters," but now we are grown up; meanwhile, science has made great progress.

Academician T. D. Lysenko. Were there no grownups then?

S. G. Petrov. It was the grownups who wrote the textbooks.

Academician T. D. Lysenko. Rokitsky's book was always wrong.

S. G. Petrov. That is absolutely true, but at one time people believed in God, they believed that the sun revolves round the earth. So it was with us.

Academician S. S. Perov. That is no justification.

S. G. Petrov. The second thing I realized was the attractiveness of building up a course of genetics on the broad conception of the Darwin-Michurin theory. I, in particular, began my first lecture with a definition of genetics and gave two definitions: one, Lysenko's definition, and the other, that of the formal geneticists. A discussion arose and the students were greatly interested. Moreover, I quoted extensive material that was connected with practice.

With every example I gave I tried to show how to single out what was positive. Recall Timiryazev's attitude towards Mendel's theory when he said that Mendel had removed the Achilles' heel of Darwin's theory. This is an example of a positive and negative attitude towards Mendel's theory. This work proved to me that the textbooks must be changed, that the very character of the teaching of genetics must be changed, and lastly, how the interest can be roused of people who have no direct connection with genetics.

A voice. Should textbooks be written with an Achilles' heel, or without one?

S. G. Petrov. Without one, of course. I think that the overcoming of theoretical errors and the proper training of young cadres will undoubtedly still further accelerate the work of improving the animals on our farms, and thereby facilitate the most rapid solution of the problem of supplying the population with the maximum of valuable animal products of high calory content.

Academician P. P. Lobanov. I call upon Academician S. S. Perov.

Academician S. S. Perov. Dear fellow Academicians. The only true trend in the science of biology is the Michurin trend, which is expressed in the concise words of our great Soviet transformer of nature, I. V. Michurin. "We cannot wait for favours from Nature; we must wrest them from her."

In the field of agriculture a similar trend was started by our Academician, V. R. Williams. His travopolye system is the foundation of foundations of the prosperity of socialist agriculture.

In the field of stockbreeding Academician M. F. Ivanov worked out a method for regulating the rearing of agricultural animals and created new breeds of animals—the Askania Merino and the White Steppe pig.

The most vivid generalization of all Michurin's principles is given in the theoretical works and practical achievements of Academician T. D. Lysenko, our President, whom I regard as the true founder and substantiator of Soviet Darwinism. I expressed this opinion at the first session of our Academy more than ten years ago.

In the modern science of biology two questions are the most outstanding:

1) What are the factors of evolution as a general biological process? and

2) What is the material vehicle of living matter, i. e., of this same biological process, with all its specific features, and primarily with the phenomenon of heredity, for that is the special distinguishing feature of living matter; in the inorganic world there is no heredity, or only the rudiments of it.

A splendid answer to the first question is given by K. A. Timiryazev in his magnificent essay *Factors of Organic Evolution*. He wrote: "Of these factors we know only three: environment which changes, heredity which accumulates these changes, and selection, which adapts, organizes and imposes on living forms that impress of perfection which presented itself as an importunate riddle from the moment man began to think." [1]

Thanks to the work chiefly of the representatives of the Michurin trend in agrobiology, in particular, the work of Academician T. D. Lysenko and his assistants, this formula of Timiryazev's has developed into a dialectical formula enriched by modern experience. This formula is as follows: the factors of organic evolution are—hereditary variation with the condition that environment is the primate; changed heredity as the

[1] К. А. Тимирязев, *Сочинения*. том V, стр. 141.

derivative of this environment; and selection as a directed and directive factor in the entire biological process in organic nature.

In opposition to this standpoint, the reactionary school, the formal Morganist geneticists, advanced the idea of the autogenetic factor in evolution. It is claimed that this factor is the form of some principle which conditions the entire process of development of the living organism independently of environment, and even of the soma of the organism itself.

Today, Academician I. I. Schmalhausen is a representative of these schools. He has published his system of views in his book *Factors of Evolution,* in which he openly says the following: "In general, the keynote of evolution was the liberation of the developing organism from the power of chance phenomena in the environment," and that "the liberation of the organism from the determining role of environmental factors means precisely the establishment of a system of internal factors of development which determine the specific trend of the form-building processes." [1]

It is evident from this that Academician Schmalhausen's standpoint in biology is that of an autogenetic indeterminist, which, of course, is one of the reactionary trends in science. Academician Schmalhausen claims to be a mutationist in the theory of evolution. He says: "Every change in the norm of reaction signifies mutation. Hence, evolution is, after all, based only on mutations."[2] But concerning mutations Academician Schmalhausen says the following: "The appearance of individual mutations is by all indications a case of chance phenomena. We can neither predict nor deliberately induce this or that mutation." [3] Such a statement shows that Schmalhausen has definitely taken the indeterminist, reactionary standpoint also in methodology.

After all is said and done, what is happening to Academician Schmalhausen is the following, to express it in his own language: the progressive complication of the system of morpho-genetic correlations, created in him (Academician Schmal-

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[1] И. И. Шмальгаузен, *Факторы эволюции*, стр. 11.
[2] *Ibid.*, p. 92.
[3] *Ibid.*, p. 68.

hausen) at the expense of elementary expressions of pleiotropism, has definitely assumed a self-regulating character. In the mind of Academician Schmalhausen, this, of course, achieves the maximum evolutionary plasticity of the organism at the expense of his, Schmalhausen's, hidden reserve of variability, mobility and adaptability to any form of thinking you like in biology. Nevertheless, it does not help Academician Schmalhausen to achieve the maximum speed of evolution of the higher organisms in the direction of dialectical materialism, for Academician Schmalhausen is hindered by individual adaptability and excessively autonomous development, although he retains a highly developed system of regulation.

Perhaps this is what deprived Academician Schmalhausen of the leadership in the field of formal, Morganist genetics, for the new star that is shining in the constellation of the Moscow State University, Professor Alikhanian, expressed in the recent discussion of Schrödinger's book *What is Life? The Physical Aspect of the Living Cell,* the plusquamperfectum of all that has served Academician Schmalhausen as a basis in the sphere of formal, Morganist (Mendelian, as Timiryazev expressed it) genetics.

It transpired that in a brief space of time there had taken place a colossal revision of classical, formal genetics, and its new prophet is already writing about the chemical nature of the gene, and the Moscow State University and the journal *Achievements of Modern Biology* (fine achievements, evidently), are even publishing it. According to this theory, the gene is merely the point of concentration at which both the external and the internal factors in the development of the organism meet. The gene does, indeed, really exist in the chromosome, but it is connected with characters and influences them only through the genohormones emitted by the genes, evidently within the organism, as hormones are supposed to do. As is known, in biochemistry, hormones are extremely complex compounds that are created by special organs—the internal secretion glands. Only a scientist who has resolved to commit scientific suicide can go to the extent of conceiving the gene as an organ, a gland, with a developed morphological and very specific structure. To conceive that the gene, which is part of the chromosome, possesses the

ability to emit unknown and undiscovered substances and, although not being a biochemist, to declare that these substances are hormones, means indulging in metaphysical, extra-experimental speculation, which is fatal to experimental science. Just as Schrödinger tried to explain life with the aid of physics, so Alikhanian is trying to explain it with the aid of chemistry. But the fate of both is an inglorious one, particularly Alikhanian's, because Schrödinger is after all a physicist, whereas Alikhanian understands as much about chemistry as Schrödinger understands about biology. The enormous literature of foreign, and rather strange, origin that Alikhanian cites in his essay on the chemical nature of the gene, although revealing his subservience to things foreign, nevertheless compels him to admit that "the direct assistance that chemistry renders in throwing light on genetic and biological evolution is as yet rather limited, but it promises to be increasingly substantial in the future." ("The Chemical Nature of the Gene," p. 105.) Exactly! It was not the joy of life that induced the geneticist to wander into a dark and foreign field from which the chemists themselves will ask him to clear out.

In another of his essays, "The Problem of the Gene in Modern Genetics," author Alikhanian quite shamelessly tried to prop up with the aid of numerous citations from the classics of Marxism an obviously metaphysical proposition like the following: "The gene is not the germ of the character, and not the only responsible material particle of the cell that determines the formation of characters, or that develops into a character. A character is the result of the development of the whole cell, of the interaction of the cells and, lastly, the result of interaction with environment [of what, after all?—*S.P.*]. The gene determines the specific development of a character, determines the direction in which a character must develop." (P. 11.) Academician Schmalhausen was right when he said that the conception of the gene "has approximated to Weismann's conception of the determinant,"[1] i.e., to the most reactionary and unscientific conception in biology.

Still more boldness, so to speak, is displayed by Alikhanian who declares authoritatively that "the synthesis of tryptophane

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[1] И. И. Шмальгаузен, *Факторы эволюции*, стр. 53.

is influenced by two different genes. One of them needs either indole or atranilic acid for growth, the other needs only indole. Both these substances are the predecessors of tryptophane, but atranilic acid is an earlier link in the chain of reactions," that "the action of the genes may be reduced to simple chemical reactions," and that "the number of genes that are connected with the synthesis of any substance is approximate to the number of links in the reaction." (*Fig. 9 and pp. 10 and 11.*)

Such is the reasoning of a man who is obviously little familiar with organic chemistry, for he persistently calls antranilic acid atranilic.

The whole essay is replete with reasoning of this kind. Clearly, the new turn in formal genetics is even worse than the old!

The chief principle of the Weismannists is the repudiation of hereditary transmission of acquired characteristics. The predominance of this conception in science always led to catastrophies before the Revolution. For example, in his book *The Biological Foundations of Zoology,* which, in general, is a very good book, that distinguished materialist biologist Professor Shimkevich, my teacher in the theory of evolution, wrote concerning Weismann something different from what he told us in private conversation; and he said, rather cynically, that he did not want to lose his chair over this question, and did not want to find himself in the position of Sechenov who, in his time, was attacked for his materialist views not only by the secular, but also by the ecclesiastical authorities. Fearing persecution under the conditions that prevailed in Russia before the Revolution, Shimkevich abandoned his interesting experiments on the influence on the process of incubation of a number of substances that cause sharp changes in the course of the ontogenetic development of the organism, check philogenetic repetition, and divert ontogenesis to a different channel.

The attacks of the formal geneticists upon E. A. Bogdanov's theoretical experiments on the meat fly, the deductions from which contradicted the tenets of Mendelism-Morganism, and on the work of M. F. Ivanov in thoroughbred cattle breeding, were so fierce, that at times they made the conditions of their work unbearable.

The formal geneticists are behaving too noisily even today in the U.S.S.R. It was enough for them to gather within the precincts of the Moscow University and find themselves in an accidental majority for them forthwith to excommunicate the Michurin trend from Darwinism, and forthwith to make an attempt to drive Comrade Prezent and Comrade Turbin from the Leningrad University. Exceptional zeal in the endeavour to annihilate the Michurinists was displayed by Professor Polyakov, from Kharkov. To characterize this professor I will read a document that dates as far back as 1927. This Polyakov wrote to Professor Kozo-Polyansky:

"Dear Comrade Kozo-Polyansky. On the instructions of a group of comrades I inform you of the following: The All-Union Congress of Zoologists, Anatomists and Histologists ended the other day. The dialectical materialist biologists who were at the congress decided to organize and to found an All-Union Association of Marxist Biologists. We have decided not to join the society for the time being, but we have established individual contact by correspondence and have included in our plan of work for 1928 an all-Union discussion conference and exchange of lectures. It was decided to enlist in our association a small group of comrades who adhere to the standpoint of consistent dialectical materialism.

"The organizer correspondents for Moscow are Levin, B. M. Zavadovsky and Agol; for Leningrad—Kurazov; for Kharkov—Finkelstein and I [i.e., Polyakov—*S. P.*]; for Tashkent—Brodsky. The Leningrad comrades have been instructed to enlist into our association the botanists who will arrive for the All-Union Congress of Botanists. We will be represented by Comrades Ryzhov and Korshikov. I think you will not refuse to join us. With comradely greetings, I. Polyakov. Kharkov, Ulitsa Artyoma 46, apt. 2, 27/XII 1927." I will not say what came of this affair. That is not so important today.

As you see, he is not a first-year factionalist. Twenty years later the desire to unite on opposition lines fired his blood again.

And, as before, the platform is of the most orthodox kind! Formal genetics under the red flag! The present attempts of the Mendelist-Morganists to organize in the field of biological science under cover of a Darwinist conference at the Moscow University are of the same nature. Not for the sake of biology—"not

very much sewing was done there, and it was not the sewing that mattered."

But no matter what bogeys these Menshevist idealists may raise to frighten me—whether talk about orthodox Darwinism, or hints at anarchism, or accusations of being a Lamarckist, etc., etc., I still think that the hereditary transmission of acquired characteristics is beyond doubt a true fact of material nature that leads to the evolution of organisms.

The second extremely important problem of biology is that of the material vehicle of living matter as a process. The formal geneticists claim to have found it in a mystical, mythical and actually non-material gene in the sex cell. I will not enter into a polemic with them in examining the absurdity of this proposition from the standpoint of biochemistry.

Academician T. D. Lysenko quite rightly says: "Heredity is *the property of a living body to require definite conditions for its life and development and to respond in a definite way to various conditions.* By heredity we mean the nature of the living body." [1] This utterance briefly characterizes what is called life, and its material factor is called the "living body." We biochemists decipher this biologically correct term biochemically, or it would be more correct to say, agro-biochemically, by the term "living protein" which is produced from "dead proteid," "protein."

Engels established that life is the form of existence of albuminous bodies, and that this form of existence consists mainly in the constant self-renewal of the chemical component parts of these bodies.

Consequently, the clue to the riddle of life, and, therefore, to the evolutionary process as a whole, must be sought in protein. Soviet science can pride itself on the fact that the substance of protein, its micromolecule, was revealed by our most distinguished scientists, Academician N. D. Zelinsky and Professor N. I. Gavrilov, who, in theory and by experiment, established the diketopiperazine structure of protein: "The micromolecule is built up of the central cyclical group of either piperazine or dihydropyrazine, with the second and fifth carbons of which various quantities of aminoacids or polypep-

[1] Т. Д. Лысенко, *Агробиология*, 1946 г., стр. 328.

tides of various length are connected in amidine-like fashion through their α-aminic nitrogen. The carboxyl of the last aminoacid is the final functional group of protein, which conditions the chief characteristic of protein as the acid advanced at one time by S. S. Perov."[1] The work of these scientists, *The Present State of the Problem of the Cyclical Nature of the Bonds of Aminoacids in the Molecule of Protein,* was in 1947 awarded the Stalin Prize. Academician Zelinsky and Professor Gavrilov write that the second part of the problem of the structure of protein, the structure of the macromolecule, has also been solved by Soviet scientists.

Thus, investigators of protein, who examined the structure of protein from different aspects, meet in the same way as two groups of workers, digging a tunnel from two ends, as a result of correct calculation, meet and shake hands.

The problem of the macrostructure of protein has been solved within the precincts of our Academy. The essence of it is expounded in the pamphlet *Casein Protein Protoacid*. As Academician Zelinsky and Professor Gavrilov wrote, this is the first standard of protein ever produced, and also proof of its acidic function. The correct solution of the problem of the macrostructure of protein enables us biochemists, or it would be better to say, agro-biochemists, to control life, and that means to understand and explain it. Examples of this are provided by the work of Perov and Chukichev on artificial plasms of the blood, when, as far back as 1931, Perov prepared the purest protein from milk-protoacid, which Chukichev injected intravenously into the blood of a rabbit; with the result that the rabbit's organism assimilated the foreign protein. Effective was the experiment of Perov and Jordansky, when protein was totally eliminated from a dog's food and introduced in the form of caseinic protoacid, i.e., the protein of milk, intravenously; the dog lived for 90 days, and the experiment on it was stopped because the assimilation of foreign protein was proved in spite of the adopted rule of anaphylactogenics and the forecasts of the formalist biochemists that the dog would die two or three hours after the experiment.

[1] Н. Д. Зелинский и Н. И. Гаврилов, „Современное состояние вопроса о циклической природе связей аминокислот в молекуле белка", *Вестник Московского Университета*, № 7, 1947 г., стр. 80.

No less interesting are the experiments conducted by N. G. Belenky, who by a very simple means transformed the serum of cow's blood and injected many litres of it into the human organism. The principle upon which Belenky's work is based proves the unity of the protein substrata after treatment under certain conditions and is one of the factors that control life.

I, in our Academy, have succeeded in solving not only the problem of the unity of protein substances, but also the problem of the specific character of protein; this has helped me to approach the problem of the specific nature of heredity and, in particular, the specific nature, if I may so express it, of the sex protein, and, incidentally, the protein of any soma, for it is specific.

The specific nature of native protein lies in a certain ingredient which is a counterbalance to the acid unity of protein, or, as we express it differently, the protein protoacid, which is common to many, if not all, plants and animals. This ingredient, which I have extracted from plant and animal substrata, is already classed as an alkaline substance, most often of the proteid type. For the time being I call it protein anticomplex. It undoubtedly differs in different native protein of the sex cells as well as of the soma. Probably we shall be able to find grounds for the difference in the varieties of plants and breeds of animals, and all the more so, in their species. At present it has been found in a number of seeds and animal plasms, zol and gel, and differ sharply in composition, belonging mainly to the histone-like protein. Some of the anticomplexes possess remarkable properties. Thus, the elimination of the anticomplex from the seeds of oats enables the quantity and quality of the sperm of the producer to be increased 50%; the elimination of the anticomplex from certain protein fodder causes a 50-60% increase in the assimilation of protein after elimination. Lambs fed on pure protein, protoacid from oats without anticomplex, increase in weight twice as fast as the control, although the ration is practically the same for both. Rats injected with pure protein simultaneously *per os* and intraperitonally, show an increase in weight of 260% compared with 100% in the control.

A large number of experiments and examples of a similar kind can now be quoted. All of them testify to the fact that

science is already able to control life, can control living and dead protein. But science cannot yet say definitely "what protein is," or "what life is" as the derivation of it. Why? Engels in his day put it excellently in *Anti-Dühring* when he said that in order to gain an exhaustive knowledge of what life is, we should have to go through all the forms in which it appears, from the lowest up to the highest.

Consequently, in order to understand and learn "what protein is," it is also necessary to go through all the forms of manifestation, from the lowest to the highest. And for this we need experiment, experiment, and again experiment, i.e., that which the Michurin trend is remarkable for, and not the speculations of formalists, who are to be found in all branches of science.

We must create a big experimental base for the purpose of studying dead and living protein, and enlist experts for this work; and this is what the Academy is doing, and will continue to do.

Long live the Academy which bears the name of mankind's genius—Lenin, the Academy of Michurin, Williams, Ivanov and Timiryazev, the Academy which is directed by the best representative of the Michurin agrobiological trend, Academician T. D. Lysenko, the Academy which is directed by the genius of our great teacher, Comrade Stalin. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician V. P. Bushinsky.

Academician V. P. Bushinsky. In its decision of July 15, this year, the Council of Ministers of the U.S.S.R. set the Lenin Academy of Agricultural Sciences a number of important tasks. These tasks follow logically from the postwar Five-Year Plan and the long-range plan for the development of our national economy. It is the duty of every worker in the Academy and in its various institutions, whether Academician or rank-and-file research worker, to push ever forward. To be able to achieve these tasks, one must be armed with an advanced, progressive theory, one must study and master all the achievements of science and technology on the basis of Marxist-Leninist methodology.

K. A. Timiryazev stated in his day that plants are the central objects of agricultural activities: "... a cultivated plant and the demands it presents—such is the fundamental scientific task of agriculture."

In harmony with him, our other distinguished scientist, Academician V. R. Williams, was of the opinion that the closest interconnection exists between plants and the soil, between plants and environment. Hence, the task of soil science and agrobiology, in the broad sense of the term, is to ensure the creation of conditions that will facilitate a continuous increase in the crop yields of our socialist fields. The soil must be given a stable fertility, it must be provided with a sufficient quantity of the factors necessary for the growth and development of plants (water, air, nutriment, useful microorganisms, etc., etc.).

In his address, Academician T. D. Lysenko dealt with the state of present-day biological science with adequate fullness. The address dealt mainly with genetics, a major branch of the science of biology. But in genetics too we get an extremely vivid picture of the struggle between two ideologies that is now raging in the field of biology.

In this struggle, we Soviet scientists must stand on the fundamental Marxian positions: to display in science partisanship, adherence to principle, innovation, an ideological approach and patriotism. In the struggle between two world outlooks one cannot occupy an intermediary position.

The most important thing in agrobiological science is the problem of changing the nature of plants, the nature of animals, and, of course, the nature of the soil. The successful solution of this problem will enable us to obtain a progressively increasing output, it will create immense possibilities for our socialist agriculture.

The founders of Marxism-Leninism proved up to the hilt that by changing nature and its laws, man can regulate the tempo and direction of natural processes, change them for his own benefit. It is not geographical conditions, not nature, that is the determining factor in the development of human society, but, on the contrary, the influence exercised by geographical environment is determined by the development of human society, by the operation of its technical and social conditions. Science itself is the theoretical side of the process of production.

Timiryazev, Michurin and Williams brilliantly proved in their works that neither the nature of plants nor the soil place any restrictions on the production of crops of ever increasing magnitude. The productivity of plants and the soil is determined by the state of agriculture and the degree to which the new agricultural techniques are employed.

In the works of Academician V. R. Williams and his school the biological trend is developed with sufficient clarity. This trend in soil science opens up wide vistas. The advanced biological trend in soil science arose in our country in opposition to the backward agrogeological and agrochemical trends that predominated in Western Europe and America at that time. The biological trend in soil science was brilliantly developed in the works of Academician V. R. Williams and Academician V. I. Vernadsky. This trend is based on the correct conception of the soil as a special body of nature that is the intermediary formation between living and dead nature. This trend alone is capable of presenting a correct conception of the soil as the chief factor in agricultural production.

One would have thought that the biological trend in soil science that was widely developed in the works of Academician V. R. Williams and his school would have roused no doubt, and would rightly have taken its place in the general complex of biological sciences. That is what we thought. And that, evidently, is what many of you present at this session think.

But T. D. Lysenko's address has shown that, unfortunately, not everything that is dictated by reason is recognized and accepted everywhere. Not long ago a debate arose because, it was alleged, V. R. Williams' theory of the fertility of the soil, the evolution of the soil and the travopolye system raised doubts in the minds of some scientists. The question these scientists asked was: should V. R. Williams' theory come within the general complex of biological sciences, or should it be included in the geologo-geographical sciences? This may seem an idle question to you, but it was actually raised.

In general, every branch of biological science that pushes forward, that looks ahead and is progressive, has been up till now met with daggers drawn in certain circles and groups of scientists. On various "scientific" pretexts, everything progressive in biological science is challenged.

The methods adopted in the struggle against these progressive trends vary. Sometimes they are merely hushed up, sometimes they are accused of being backward and useless, and sometimes there is talk about their being purely an applied science, and a search is made for a place to which this science can be sidetracked.

There are still circles among us which adhere to the old-fashioned division of science into the profoundly "academic," theoretical, and applied. And the opinion is held that the place of the applied sciences is the Lenin Academy of Agricultural Sciences. But this Academy must deal with theory no less than, for example, the Academy of Sciences of the U.S.S.R. The function of the Academy of Agricultural Sciences is to work out the theoretical principles for producing high and stable crop yields. But tell me, how is it possible to work out the theoretical principles of agricultural science if those branches of knowledge that serve as the biological foundation for producing high and stable yields are not widely developed? Hence, it is no accident that a report on the situation in the science of biology has been made at this session.

I come back to the question of the soil. Being a natural body, the soil is at the same time the bearer of fertility. The soil is the chief means of production, the soil is a product of labour, and this is the most important. But the soil must not be regarded only as an "inert," lifeless substance of the biosphere, like rocks, subsoils and various chemical compounds. Unlike dead rocks, the soil is a "bioinert body," and is the product of the interconnection of life and environment.

As a result of the development of biological and biochemical processes, the upper parts of every soil-forming rock, under the influence of the joint action of higher and lower organisms, acquires new characters and properties. Such upper parts, or levels, formerly the "inert" parts of the biosphere, are gradually transformed into soil, into "bioinert bodies."

This conception was developed by V. R. Williams and finds expression in the theory of the single process of soil formation, in the theory of fertility, in the substantiation of the theory of the travopolye system.

One would think that everything was clear here, but in this field too we have to fight. V. R. Williams showed very clearly

that at the present stage of development of agriculture a definite type of soil cultivation must be proposed. As is known, this was the travopolye type. In it, theoretical problems are most closely interwoven with profoundly practical problems. And we know that at the present stage of development of agrobiological science the theory of the travopolye system is already beginning to occupy a firmly established place. The travopolye system of soil cultivation is becoming an indispensable and inseparable link in the process of agricultural production in our country.

V. R. Williams showed that the cultivation of the soil must not be regarded, as it was formerly, merely as an agrotechnical operation. It is an extensive, soil forming process, which can change any kind of soil. Developing V. R. Williams' theory, we are of the opinion that the cultivation of the soil is a process that takes place under the strong influence of man's production and economic activity. The soil is the product of human labour; consequently, soil can be made. All this brings us to the serious task that confronts soil science, namely, fundamentally to change the nature of the soil. Our task now is to study not the static state of the soil, the soil as it is, but the dynamic environment that is created under the influence of the development of cultivated plants, by crop rotation. Our path must lead us to the transformation of the soil, to the development of new processes in the soil that will ensure high, stable yields, high and stable fertility.

The travopolye system consists not only in a series of agronomic measures, but also in the combination of the various branches of plant cultivation and animal breeding. At the present stage of development of science, the theory of soil fertility and the travopolye system of cultivation will help us to carry out the fundamental task that confronts our agriculture, namely, to increase the yields of agricultural crops and the productivity of stockbreeding.

It must be added that the travopolye system involves also technical measures, namely, mechanization, chemicalization, and land reclamation, measures which will facilitate the creation of favourable conditions for the growth and development of plants.

Developing V. R. Williams' theory, we, against the background of the travopolye system, have worked out an additional

measure which we call the radical transformation of the nature of the soil. I will not deal with all the aspects of this problem here; I will merely observe that by more resolutely changing the nature of the soil we can, in the course of a few years, convert the soil of the northern non-Black-Earth zone, despite all the most unfavourable properties of its upper levels, into a highly fertile soil. Basing ourselves on the biological proposition that the soil is the product of the interaction of plant formations and the dead substratum, we can create new soils that will be superior to the turf-podzol soils.

Wide opportunities are opening for soil science. Our task is to take quick advantage of these opportunities. It is not always easy to do this; often, instead of support, we meet with indifference, inertness. But life is pressing forward and, of course, all obstacles will be overcome.

The victory of Socialism in our agriculture is creating inexhaustible opportunities for the subordination of nature to man, for achieving the highest productivity of labour, the highest level of agriculture. Collective farms and state farms provide the only possible basis for putting into practice all the grand designs and bold innovations of science and technology. And we scientists must in our daily activities remember that only close and manifold connection with practical, socialist agriculture will enable Soviet agricultural science to achieve new and bigger successes.

Our science, jointly with the Stakhanovite front-rank workers in agriculture, with the front-rank kolkhozes and sovkhozes, will rapidly put into practice the ideas of Darwin, Timiryazev, Michurin and Williams. The purposefulness of our science is higher than that of any other in the world because it is stimulated by the struggle for Communism. (*Applause.*)

Academician P. P. Lobanov. I call upon Doctor of Biological Science, J. A. Rapoport.

J. A. Rapoport (Institute of Cytology, Histology and Embryology of the Academy of Sciences of the U.S.S.R.). This session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. imposes a great responsibility upon Soviet scientists. We represent the science of the Soviet State. We are putting theory into

practice that is based on new, unprecedented forms of social life.

Naturally, our theory, which penetrates Soviet reality, is carried into the rural districts, must be on a high level. Our science and practice must be superior to the science and practice of capitalist countries. We must be clearly convinced of the correctness of the principles we choose for our practical activities and not fear criticism, not be afraid to admit mistakes, and not fall into the habit of indiscriminately glorifying our achievements, or of overrating what has been done.

The President of the Academy has delivered an address on a very wide subject that will serve as the program of our activities for a long time to come. It was an address on the situation in the Soviet field of biology and on the prospects that are open for us for a very long period ahead. We must therefore pay careful attention to the criticism to which Comrade Lysenko subjected certain branches of the Soviet science of biology, in particular, the theory of evolution and the theory of heredity, i.e., modern genetics.

The very need of certain mechanisms to fix the variations that have taken place, no matter how, calls for very exact scientific experiment. Genetics tries very hard to solve this problem by conducting experiments and by weighing up the material obtained from the experiment and corresponding control. Naturally, the various hypotheses that arise in the mind of the experimenter often contradict the theories that are current in the broad field of science. Truth emerges from conflict.

Thus, the modern theory of light is the fruit of a conflict between theories—the wave theory and the corpuscle theory. During this conflict, first one view and then another triumphed, and any restriction of opportunity or excessive severity towards theory would have been prejudicial to the interests of science. In Soviet theory we are remote from the idea of suppressing any fruitful point of view.

The foundation of genetics, as the very name indicates, is the gene, the material vehicle of heredity. And the theoretical controversy that is raging around this problem mainly concerns the gene, of course.

The founder of the modern theory of the gene was Charles Darwin. To be convinced of this it is enough to read several

chapters of his *Variation of Animals and Plants Under Domestication,* chapters that are not the chance fruit of a great man's imagination, but the result of twenty-seven years of research. Without the recognition of a material base, the theory of natural selection could not, of course, exist.

It is wrong to think that Bateson is an adherent of the theory of the gene. I will take the liberty of referring to Bateson's published works of 1926. There he said that he does not believe in the theory of the gene, that he does not think that the gene exists.

The same was said by Johannsen, who quite definitely stated that the theory of the gene has no real ground to stand on.

This was also the standpoint of Lotsy, who categorically stated that there is no such thing as mutation, and that the gene as a material unit also does not exist.

Many of these authors totally deny that there is a connection between the gene and the chromosome. Naturally, every idealist, no matter what branch of science he may be working in, or what creed of philosophical science he may be preaching, must give something in place of the theory which is sometimes unjustly subjected to hasty criticism. And indeed, other proposals are advanced. Such, for example, is the proposal to ascribe heredity to psychical factors. This theory is advanced by a number of Western scientists—Semon and others. It is characteristic of many idealists of the type of Driesch, of many Lamarckians of the type of Cope, and of other scientists who adhere to the standpoint of consistent Lamarckism.

The theory of memory and requirements was accepted by the idealist philosopher Mach, who studied problems of heredity, and even conducted experiments on many animals. He said that heredity can be attributed only to requirements that went beyond the limits of matter, and our new Academician Prezent agrees with this.

The gene is a material unit of enormous molecular weight of the order of hundreds of thousands and even of millions of units. There are genes in the nucleus of the cell at very definite points of the complex structures which are called chromosomes. These units became known to us as a result of persevering and laborious experiments. We became convinced that the units can be artificially shifted from one chromosome system to another. We became convinced that these hereditary units—genes—are

not immutable, but, on the contrary, are capable of undergoing mutation.

Mutation is an immense achievement of Soviet science in that it revealed the powerful operation of external physical factors and the operation of chemical factors. In the work, which Academician Perov referred to here so scornfully, great difficulties were overcome and definite results were achieved. These achievements are that we Soviet geneticists have found chemical agents which enable us to obtain at will hereditary variations many thousand times more often than was the case before. These are chemical compounds that cause hereditary variations in every sprouted fungus cell.

As a result of this work we can say that we have utterly refuted Weismann's proposition that the germ cell is contained in a separate case. There is no such case, for the germ cells change with the same frequency as the body cells.

There is no such case, and we are able to change the material substratum of life, we can actively make the genes what they ought to be. That there is no such case is also proved by the fact that embryology has definitely shown that the sex cells do not differ from the body cells.

We are now on the verge of great discoveries in the sphere of genetics. Many of you remember the discovery of the bacteriophages, the minute viruses that live as parasites in bacteria. Flying in the face of numerous facts, many scientists denied the existence of bacteriophages right until very recently. Today, the colossal development of microscope techniques enables us to see the bacteriophages of the dysentery cell, cholera bacteriophages, phages that cause various intestinal diseases in domestic animals. Thus, both veterinary and medical microbiologists can see that the claim to the existence of special, invisibly minute material units postulated on the basis of propositions not yet directly proved was justified: such a unit actually exists. It is possible to see the minute structure of the bacteriophages, to see how they penetrate the cell, propagate, burst its membrane and cause its destruction.

The gene is a still more mysterious unit, still more remote from the possibility of ocular demonstration, but at any rate it is a material unit in respect to which it is possible to attain big practical achievements. It seems to me to be a great practi-

cal mistake wholly and indiscriminately to deny the immense achievements of Soviet genetics. It is our duty to draw a distinction between Soviet and bourgeois genetics. Soviet geneticists have never taken a wrong, anti-Darwinist stand. They have linked natural selection, which reasonably and rationally explained the phenomenon of development of organic life, in a single great principle.

Genetics has described some of the mechanisms of obtaining to some extent directed variations by the repetition of a definite experimental procedure. Thanks to this, genetics can be of productive service to our socialist agriculture. It can serve it by utilizing on a vast area planted with maize the method of heterosis, which, to our shame be it said, is not being sufficiently employed in agricultural practice in spite of the obligatory decision of the Plenum of the Central Committee of the C.P.S.U.(B.) of February 1947. This method will permit the yield of maize crops to be increased by 25%. This is not a figment of the imagination, but a definite fact, and this method must be utilized. It can be applied to a number of other plants. Sugar cane, castor-oil plant, and other plants, show a positive response to this method. The heterosis method will enable us to increase the production of the proteins, fats and carbohydrates that our national economy needs.

Nor do we make adequate use of the artificial polyploid method, which we vulgarly call colchicine treatment, but by the aid of which we obtain a double unit of heredity. We can see kok-saghyz, tau-saghyz, sunflower, hemp and other plants the size of which is twice that of the initial diploid plants. Hundreds of similar examples can be quoted in the case of decorative plants. Nevertheless, there is no sign of that perseverance which is necessary to squeeze the utmost out of the polyploid method. This method is important because of its practical potentialities, but its theoretical significance is also great. It shows that it is possible to produce by human hand species which took an immense length of time to create in nature (tobacco, plums).

Genetics can be of immense service to veterinary microbiology in that it enables us to produce species with a shattered pathogenical system. We can produce species of bacteria that will not cause disease but cause immunity ("living vaccines").

This has been done by many scientists who have devoted years of labour to the task of protecting mankind from tuberculosis, rabies, and a number of other dreadful diseases. At that time these were examples of chance discoveries. Today, the possibilities for such work are much wider. If microbiology critically accepts the positive core of genetics it can now make it serve our socialist society.

I think that biology will develop on the basis of the wide application of the principle of natural selection, which is incompatible with Lamarckism, which contradicts Lamarckism. Lamarckism, in the form in which it was rejected by Darwin and accepted by T. D. Lysenko, is a conception which leads to errors. Tens of thousands of precise experiments have convinced us that it is impossible to change animals and plants solely by the dictates of our desire. We must know the mechanism that lies at the base of definite morphological and physiological properties. Only by knowing these mechanisms can we succeed in changing organisms. Even Michurin, whose name is repeated here so often, pointed out that we must not confine ourselves solely to training in the broad sense of the term, but that we must also utilize more active methods—selection, hybridization. And the entire army of Soviet biologists stands by the theory of selection which Michurin utilized in all his works.

Michurin repeatedly pointed to the possibility of extensively utilizing genetics not only in horticulture, but also in field culture. He urged young people to study genetics.

This was long ago. Since then genetics has made much progress, and I cannot agree with those comrades who demand the elimination of the genetics course from the curricula of our colleges and universities and the rejection of principles on the basis of which valuable varieties and breeds have been and are now being produced.

We must not simply ape others, but it is our duty critically and creatively, as V. I. Lenin taught us, to assimilate all that has been done abroad. We must carefully tend the shoots of what is new, and train new cadres who will be able to push science further forward.

Only on the basis of truth, by criticizing one's own mistakes, can we achieve in the future the big successes our country is calling for. (*Some applause.*)

A voice. What is your present attitude towards the question of the inheritance of acquired characteristics?

J. A. Rapoport. I think that the internal mechanism of genic action consists in that the gene, each gene, in essence, corresponds to one definite enzyme, to one definite enzyme system. This has now been shown by a number of experiments on several organisms of a lower order—bacteria and fungi. These researches are now of great practical significance, and a big step forward has been taken in this respect.

It may be demonstrated that a physiological character also changes as a result of mutation, because there is, of course, no such thing as form divorced from material content. It is possible to produce a change in a definite direction due to the exclusion of this or that enzyme system. It is the enzymes that are directly responsible for the particular modifications. These enzymes are well known to biochemists, with whom geneticists maintain close connection and will undoubtedly maintain closer connection in the future. This is the school of Academician A. N. Bach and Academician A. I. Oparin. Here it is perfectly evident that if we operate on an organism with an enzymatic poison, say, we will get a definite modification which will give rise to a new character. Consequently, the mechanism of modification is the mechanism of the action on the enzymes, or on some other equally important units. It is very easy to obtain these characters, because the molecular bond here is of quite a special kind.

Mutation is another thing, it is an irreversible variation. Here a new molecular bond is established, and the variation that is obtained is transmitted by heredity. In this connection it must be clearly understood that it is possible to operate on the external system, on the membrane, on the enzyme system, and easily obtain variations in characters, the non-hereditary system, but there is no connection between a variation in the gene and modification of this kind, as the Lamarckian theory postulates.

Thus, it must be admitted that there is a separate system of modifications and a system of mutations. We are able to direct both systems, and this will be more fully proved in the future, for genetics is on the threshold of great discoveries.

Academician P. P. Lobanov. I call on Comrade G. A. Babajanyan.

G. A. Babajanyan (Director of the Institute of Genetics of the Academy of Sciences of the Armenian S.S.R.). Comrades, I am in a better situation than preceding speakers because I do not have to quote from books. I will deal with what Doctor Rapoport said.

Doctor Rapoport said: "Soviet geneticists have never taken an anti-Darwinist stand." What do our Morganists expect to gain by making a statement like that? It is the same as saying that our Morganist geneticists have never taken a Morganist stand. Who, if not Morgan, in his works, regards Darwinism as a system of speculations on problems of evolution, a system devoid of all experimental foundation?

Who does not know that Johannsen, one of the founders of Morganistic genetics, was a most typical anti-Darwinist? In his works, Johannsen quite openly opposed Darwin. It is not what Johannsen said that matters, however, but the substance of his theory. Who is not aware of the nature of Johannsen's metaphysical pure lines theory?

It is impossible to conceive of a more outstanding anti-Darwinist than Weismann. The chromosome theory of heredity is, in substance, Weismann's idealist theory of the immortal germplasm. It was said here that the Morganists do not share Weismann's views, but Rapoport's entire speech was based precisely on Weismann's arguments. In his address Academician T. D. Lysenko recalled that N. K. Koltsov, who directs the institution at which Dubinin and Rapoport are working, asserted that the soma, i.e., body cells, are the very opposite of the sex cells, and that the mortal body is merely a case for the germ cells. Of course, a researcher may not share the views of his Director, but from Rapoport's answer to the question about the hereditary transmission of acquired characters it follows that the organism possesses a separate system of mutations and a separate system of modifications. And after that he asks: "Who says that we are Weismannists?" But is not what Rapoport said about mutations and modifications Weismannism? It is the purest, undisguised Weismannism. A distinct system of mutations, a distinct system of modifications—who does not know that this

is precisely anti-Darwinism? The conception of the existence of a separate chromosome "apparatus" (the Morganist definition), distinct from the modification "apparatus," is precisely an anti-Darwinist, a Weismannist conception. Mendelism-Morganism rests entirely upon what by their very nature are the anti-Darwinist theories of Morgan, Johannsen and Weismann.

It was rather difficult to understand some parts of Rapoport's speech. In one part of it he elaborated the idea that the gene is as yet a hypothetical material unit, that the physical existence of this hereditary substance has not been proved. But in another part he said that we have the gene in our hands. This is interesting—the gene is invisible, but the Morganists have it in their hands.... (*Laughter. Applause.*)

Rapoport said that Morganist genetics is hastily criticized, that hasty conclusions are drawn about Morganism. But you know, these people have queer ideas about haste and patience. The character of the two conflicting trends has been discussed in our country for twenty years, and after that people come along and say that "hasty" conclusions are drawn about Morganism-Mendelism. What do they hope to gain by statements like that? They hope to gain time. With this object in view they keep on promising to discover new substances that will produce mutations. One of them, said Rapoport, has already been discovered.

We have been hearing about substances that produce mutations for quite a long time. We remember with what aplomb and cocksureness the Morganists spoke when X-rays, ultra-violet rays, ammonia, formaldehyde, etc., etc., were first employed as mutation producing factors. Do you want us to wait another 20 years to learn the nature of your new chemical mutagenic substance? We are told that this chemical substance is already causing a large number of mutations. That sounds good—a large number of mutations. But what does it really amount to? It would be better if this "large number" did not exist, because the organisms obtained in this way are all trash, freaks! Rapoport could not prove that the new mutations they have obtained differ in any way in principle from the innumerable mutations they obtained before. In his *Factors of Evolution,* Academician Schmalhausen gives a list of an enormous number of unviable mutations. What grounds have we for thinking that the new

mutations obtained under the influence of the new mutagenic substances are of a different nature? On the contrary, we have every ground for thinking that they are of the same nature.

Lastly, let us assume for a moment that a small number of not harmful, not lethal, but useful mutations have indeed been obtained. But who wants them? Who wants what by their very nature are useless *Drosophilas*?

J. A. Rapoport. But there are useful mutations, and many of them. Why do you shut both your eyes to them?

G. A. Babajanyan. Firstly, because they are useful mutations for a useless object. (*Applause.*)

J. A. Rapoport. We have cures for tuberculosis and other diseases.

G. A. Babajanyan. You only make promises.

J. A. Rapoport. And you promise to produce strains in two years, but don't keep these promises, and you don't admit your mistakes.

G. A. Babajanyan. Rapoport says that they apply their theory to practice. What theory do you apply to practice? By its intrinsic nature your theory is directed against practice. Your "theory" is not only indifferent to, neutral towards, practice. In this respect it is strange to hear even some Michurinists say that Mendelism-Morganism is "divorced from practice," "has no connection with practice." We must say very emphatically that the Mendelist-Morganist theory is inimical to practice. The very foundation of Mendelism-Morganism is the principle that biological laws are unknowable. For twenty years the Mendelist-Morganists have opposed all the discoveries, all the achievements of Michurin agrobiology, they have opposed all the innovations of Academician Lysenko. The Mendelists are not only enemies of the established and proved achievements, but also potential enemies of all future achievements. (*Applause.*)

From this standpoint Mendelism-Morganism is the vehicle of idealistic agnosticism in biology (*applause*), proclaiming in principle that biological laws are unknowable.

In his address Academician T. D. Lysenko gave us a profound analysis of the situation in the field of biology. In our country, on the basis of the development of Michurin's theory, Darwinism has been transformed from a theory that

explains the evolutionary origin of the organic world into a mighty theoretical foundation of biological science, into a harmonious system of effective, fruitful knowledge about the development of the living world. The Michurin science of biology alone, based as it is on the principles of dialectical materialism, could raise the problem before which Darwinism halted, and which Mendelism-Morganism proclaimed insoluble, namely, the problem of the causes of variation, of the laws that govern the rise of hereditary variations, of the methods of directed alteration of the heredity of organisms. And the Michurin science has achieved considerable successes in this most difficult but most important problem of biology.

The theoretical propositions of Michurin biology spring from a vast mass of experimental data and have been tested by extensive practice. This explains why these theoretical propositions are really scientific, and not inventions, why they objectively and correctly reflect natural laws and possess immense constructive, creative power.

Indeed, who does not know that as long as the nature of "winterness" and "springness" was studied independently of the requirements of production, independently of the requirements of plant breeding, independently of the endeavour to direct the vegetative period of plants, it was really impossible to know the causes of the variation of plants as regards "winterness" and "springness"? The presentation of the problem of the nature of "winterness" on practical lines led to an immense scientific discovery, to the discovery of the laws of the phasic development of plants.

Our native Mendelist-Morganists, together with the physiologists of the factorial trend, as is to be expected from real conservatives, refused, and now refuse, to resign themselves to the proposition that, whereas it was impossible to direct the hereditary variation of winter plants into spring plants, and vice versa, before the discovery of the phasical development theory, it is now quite possible to direct this hereditary variation.

Instead of taking up these new facts and approaching the achievements of our country's science with due respect and objectivity, as becomes truly progressively thinking scientists, our Mendelist-Morganists declared war on the new. And they

are even waging this war when, one would think, the enormous number of experiments that confirm the truth of the laws that have been discovered should have convinced them of their errors.

Meanwhile, the theory of phasic development has opened splendid opportunities for biological research. It has laid a firm theoretical foundation for an enormous number of accumulated but not yet understood facts in physiology, plant breeding and genetics, and has opened wide opportunities for the development of these sciences.

The Mendelist-Morganists flew in the face of facts because Academician Lysenko's theory of phasic development, which was an immense discovery in Michurin biology, exposes the idealistic principle that the living organism is independent, autonomous, of environmental conditions, exposes the chromosome theory of heredity, i.e., Weismann's idealistic theory of the immortality of the germ-plasm under a different name. The Mendelist-Morganists are opposed to the theory of phasic development because it exposes all the hypotheses and "theories," the object of which is to prove that it is impossible to know the causes of variation, that directed alterations in the living organism are impossible.

And our Mendelist-Morganists refuse to reckon with facts and prefer to remain captive to idealistic Weismannism, to idealistic biology, prefer to remain a bellicose, aggressive detachment of the reactionary trends in biology that predominate abroad.

It is well known that from the practical point of view, Mendelism-Morganism is good for nothing, useless, sterile. It is well known that for this reason the Mendelist-Morganists, and the scientific institutions at which they work, have no connection with practice, are divorced from life, from production. But in our country Mendelism-Morganism is not simply a trend divorced from production, from the requirements of agriculture, it is not simply neutral and indifferent. Mendelism-Morganism is aggressive towards the achievements of Michurin biology, it is hostile towards them.

In this sense it is strange to hear people say that Mendelism-Morganism denies the possibility of directing what is called "segregation." What does Mendelism-Morganism not deny? It

denies everything. It denies after the facts have been obtained, and denies beforehand that it is possible to obtain such facts. Impossibility, unknowability, absolute agnosticism—such is the intrinsic nature of Mendelism-Morganism, the cause of its absolute sterility.

How otherwise can you explain the fact that as soon as a new discovery is made our Mendelist-Morganists rise up in arms against it, against the new knowledge, against the new methods. At first they simply deny. Then they try to give their own interpretation, and after that they perseveringly strive to obtain an effect opposite to the one that was obtained as a result of this or that influence. And innumerable researches are undertaken in "devernalization," and a whole "problem" of the reversibility of biological processes even arises.

In order to give you at least a slight idea of how widespread researches of this kind are in the Soviet Union, I must tell you that in Armenia we have physiologists who have exhausted all the possibilities of "devernalization." One of them, the most persevering, decided simply to pour hot water over vernalized seeds in order to devernalize them. And to give this "research," save the mark, a scientific appearance, he defined his work as research into "the reversibility of the phases of vernalization under the influence of a hydro-aero current." He worked on this for many years. Had he been brought up in a milieu in which the theory of vernalization and not devernalization is presented as a scientific achievement, had he been free from the weight of authority of people who have gained notoriety in their struggle against the achievements of Academician Lysenko, he would have known the simple fact that non-reversibility of phasical processes is a law, that non-reversibility is an immense achievement of organic evolution, which ensures the qualitative constancy of forms under variable conditions of life.

The Mendelist-Morganists opposed the method of intra-varietal crossing, on the basis of which the theory of elective fertilization arose. An organic part of the Michurin-Lysenko teachings, this theory has been and is of immense service to our plant breeders and seed cultivators. The theory of elective fertilization has thrown light on the extremely important problems of maternal and paternal heredity, of so-called

"segregation" or the phenomenon of hybrid multiformity, on the methods of obtaining constant hybrids, on the nature of heterosis, or phenomena of hybrid vigour, and on many other fundamental problems of genetics.

On the basis of the theory of elective fertilization, methods have been elaborated such as intravarietal crossing, the mass production of intravarietal hybrids, artificial pollination of cross-pollinating plants, and many other methods of improving the work of plant breeding and seed cultivation.

As time passes we become more and more convinced that T. D. Lysenko's idea of fertilization, of the sexual process, is of enormous cognative importance and helps us to discover the chief ways and means of directing these vital biological processes. I will quote one example of the work of the Institute of Genetics of the Academy of Sciences of the Armenian S.S.R.

In Armenia there is a widespread population of wheats consisting of varieties historically adapted to each other and to the conditions of constant cultivation. Evidently, the conditions of fertilization of wheat in population differ from the conditions of fertilization of pure lines. The study of hybrid plants selected for the purpose of producing varieties under these conditions of pollination and fertilization gave positive results. Data for four years of variety testing in the case of one strain, and data for two years in the case of two strains, show that the new strains are always superior to the local standard varieties. For some years the superiority amounts to 6-14 centners per hectare. These, our first born in the work of plant breeding, were produced by hybridization methods. They were obtained on the basis of the elective fertilization theory, and they confirm the immense practical significance of this theory.

Only when we know that fertilization is not the chance combination of individual gametes, when we know that there is community between sexual and vegetative hybridization, when we know that fertilization, as well as other vital processes, is a process of assimilation and dissimilation, in other words, only when we are familiar with the Michurin-Lysenko theory of fertilization, can we, in such researches, discover new phenomena, new laws.

Rye, as is known, is a strict cross-pollinator; isolation leads to utter sterility, and inbreeding to depression in the progeny. In one of the experiments that we have been conducting at our Institute for the past three years and more, self-fertilizing rye plants in isolators were given the pollen of spring wheat. In a number of cases this led to a heightened grain formation. In 1948 one plant by this method showed 20% of grain development compared with only 1% in the case of pure inbreeding; another plant showed 22% of grain development in the presence of foreign pollen and 0 under pure inbreeding; a third plant showed 24% and 0; other cases showed 33% and 2%, 39% and 0, 48% and 0, and 54% and 0. This is the highest figure we have obtained. Thus, such strict cross-pollinators like rye, which produce no seeds with ordinary inbreeding, produce them under the influence of foreign pollen.

But this is not the most important, although this in itself is an indication of the diminution of the depression of self-pollination. The important thing is that in plants produced in this way the depression of inbreeding is diminished, and in many cases it disappears in the progeny. From the point of view of the Mendelist-Morganists, researches of this kind lead to paradoxical phenomena.

The solution of the problem of inbreeding must proceed not through the absolute isolation of plants, but by breaking down this isolation by utilizing a foreign pollen which takes an incomplete part in the act of self-pollination. Nature abhors isolation and in an infinite number of ways creates pollen mixtures on the stigma of plants. This mutual influencing in the act of fertilization plays a part in the rise of viability of pure breeds in natural surroundings, plays a part in protecting strains and populations from the destructive action of pure pollination, of inbreeding.

In conformity with the Michurin-Lysenko mentor theory, and bearing in mind the similarity of the reciprocal influence of the vegetative and sex cells, this phenomena may be regarded as the result of the action of the sex mentor. Quite 12 years ago, T. D. Lysenko uttered a warning against the employment of the absolute isolation method and denounced the Morganist theory of inbreeding, which for many years had hindered the solution of this problem. It is known that, accord-

ing to this theory, inbreeding is merely a harmless factor which protects the genotype from the lethal and semi-lethal genes, the mission of which, for no accountable reason, is to kill the organisms, their own bearers.

Instead of admitting the facts which speak in favour of crossed pollination and show the harmfulness of prolonged self-pollination, instead of rejoicing at the new discoveries which help us to organize seed cultivation on proper lines, to increase crop yields, and solve the most difficult problems of genetics, the Mendelist-Morganists opposed the Michurin biologists, opposed Academician Lysenko with amazing tendentiousness. And they did so because the new facts contradicted the views of the Austrian, amateur plant experimenter Mendel, contradicted the opinion of the English conservative scientist Bateson, the metaphysical theory of the anti-Darwinist Johannsen, and of the American scientists East, Shull and Jones who invented this preposterous "conception," save the mark, of inbreeding.

But the reactionary nature of the "theory" of the Neo-Darwinists was most vividly revealed in the problem of intraspecific competition. It was enough for Academician Lysenko, on the basis of his experiments with cluster planting, to raise the question of purging Darwinism of the mistakes of Darwin, of Malthusianism, of the reactionary idea of intraspecific competition which has been fostered in biology on unscientific grounds, for all our native Neo-Darwinists to rise up in a body against him.

And so, step by step, persistently and methodically, the Mendelist-Morganists have been for 20 years opposing literally all the discoveries of Michurin biology, literally all the innovations of Academician Lysenko. In this struggle they rely on their numerous representatives in our colleges and scientific institutions, from individual laboratories to Academies.

In Armenia we have the State Plant-Breeding Station, the Institute of Industrial Crops, the Institute of Agriculture, the Institute of Stock Breeding, the Vinegrowing Institute, a zonal fruit and vegetable station, and agrochemistry, soil science and other institutions. But these are not coordinated organizationally and lack central scientific-methodological guidance. Nor is there a directing centre to control and coordinate the

activities of these numerous institutions, which spend millions of rubles on their work. And the chief thing is, there is no central direction of the scientific research themes that are dictated by the specific requirements of our economy.

It is not surprising therefore, that these so-called "academic" institutions stand aloof from such applied science institutions as plant-breeding stations, or the Institute of Industrial Crops. This indifference persists even when it becomes clear that these institutions cannot cope with their tasks by their unaided efforts. And things have been going on like this for years. Unfortunately, even the central institutions to which some of the scientific organizations indicated are subordinated, are evidently unable to give them the guidance they need. All this creates conditions which permit the work of these institutions to proceed in a wrong direction, and leads to the arbitrary choice of themes, sometimes to suit the personal tastes of the scientists concerned.

This state of disunity in the organization of the Academy of Sciences of the Armenian S.S.R. is reflected in the independent existence of two departments: a biological department and a department of agricultural sciences.

To ensure the further development of the agricultural sciences in our Republic, all these institutions must be united to form a branch of the Lenin Academy of Agricultural Sciences of the U.S.S.R. This branch must become the guiding centre for Michurin agrobiology in the Armenian S.S.R.

This session of the Academy, discussing Academician T. D. Lysenko's address, can place on record the immense achievements of our Michurin biology. There can be no doubt that these achievements would have been much greater had not the Mendelist-Morganists, Neo-Darwinists, and all the other representatives of reactionary, idealist, pseudo biology, directed all their efforts against them for many years. Nor can there be any doubt that by the decision it will pass, this session will open new opportunities for the further development of the Soviet agricultural sciences. (*Applause.*)

Question from the hall. If you think that self-pollination is harmful, how do you account for the existence in nature of highly developed self-pollinating species?

G. A. Babajanyan. I am not the only one who thinks that self-pollination is harmful. Sufficient facts have been accumulated which prove the harmfulness of self-pollination. As a result of his experiments, Darwin obtained an enormous number of facts that proved this proposition. Michurin biologists have also accumulated an enormous number of facts which prove the harmfulness of prolonged self-pollination. The existence in nature of self-pollinating forms is a beneficial biological factor, although it is possible to conduct experiments in the direction of proving that, if no complete cross-pollination takes place among these species, the influence of foreign pollen, however, does to some extent neutralize the harmful effects of prolonged self-pollination.

Academician P. P. Lobanov. With this we close today's sitting. The proceedings will be resumed tomorrow at 11 a.m.

FOURTH SITTING

Morning, August 3, 1948

Academician P. P. Lobanov. We will continue the debate on Academician T. D. Lysenko's address. I call upon Academician A. A. Avakian.

Academician A. A. Avakian. Comrades, one of the fundamental problems of biology has been in the past, and is today, the possibility of directed changes in an organism in conformity with the influence of changed environmental conditions, and of the inheritance of characters thus acquired. The problem is: does the heredity of living organisms change in conformity with the changes in the body of the organism, or do changes in the body not affect the inherited characters?

The reactionary trend in the science of biology—Mendelism-Morganism—from its very birth (Weismann, Bateson, Johannsen, Morgan and others) was directed against Darwinism. This pseudoscientific trend continues to this day to oppose creative Darwinism—the Michurin theory.

Such, for example, are the foreign Morganists Sax and Darlington, and such are our native Weismannists—Academician Schmalhausen, Professor Zhebrak, Professor Dubinin and others.

We regard as absolutely erroneous the assertion of Academician I. I. Schmalhausen that the science of biology developed through the theories of Weismann, de Vries and Morgan. Actually, the genuine science of biology developed in the course of the struggle against reactionary Mendelism and Morganism. In his textbook *Problems of Darwinism,* published in 1946,

Academician Schmalhausen writes: "... while our appraisal of A. Weismann's theoretical concepts may, perhaps, be severe, we must not forget his great positive significance in historical perspective, at a certain and from us not very remote stage in the development of biology. Weismann's views served as the basis of a new science—genetics."[1]

To what stern criticism has Academician Schmalhausen subjected Weismann's theory, since he apologizes to Weismann so humbly?

On the preceding page we read the following:

"At one time these concepts [i.e., Weismann's concepts—*A.A.*] were of great progressive significance."

"The concept of the organism as a mosaic of independent characters and qualities determined by hereditary units that are independent of each other, also proved to be an extremely fruitful working hypothesis. . . ."[2]

"The enormous achievements of the whole of the first stage of the development of genetics were conditioned by this concept of Weismann's. . . ."[3]

This is what Academician Schmalhausen wrote in his textbook on Darwinism in 1946. Schmalhausen knows very well that the Morganists, the followers of Weismann, have not discarded Weismann's principle of germinal selection. This can be seen from the work of Docent Alikhanian of the Moscow University, who, in an essay entitled "The Chemical Nature of the Gene," writes:

"This specialization, however, is subject to evolutionary control exercised by the genes, by the natural selection of the genes, because the action of the cytoplasm is itself regulated by the genes."

No, the Weismannists abroad, as well as here at home, have remained faithful followers of Weismann. Actually, biology was developed by the works of Lamarck, Darwin, Sechenov, Pavlov and Timiryazev, by the works of the great Soviet scientists I. V. Michurin, V. R. Williams and T. D. Lysenko, who have still more profoundly developed the materialistic core of Darwinism.

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[1] И. И. Шмальгаузен, *Проблемы дарвинизма*, 1946 г., стр. 200.
[2] *Ibid.*, p. 199.
[3] *Ibid.*, p. 200.

We Soviet biologists must regard Academician Schmalhausen's defence of the views of Weismann and Morgan as open defence of the reactionary trend in biology.

Academician Schmalhausen regards the process of evolution as a process of autonomous development of organisms divorced from the environmental conditions of life. He writes: "Progressive autonomization of development means, therefore, the substitution of the external factors by the internal factors of development and, at the same time, the stabilization of forms."[1]

The "Darwinist" Lukin, a follower of Academician Schmalhausen, writes that the more firmly a character becomes hereditarily fixed, the less specific is the influence of environmental conditions upon its development, and the more does the autonomy of the gene's action grow. As has been explained, writes this "Darwinist" further on, a character that is not hereditary is produced "in the course of things," i.e., it develops if the external factor influences the development.

This pseudoscientific, anti-Michurin proposition of the Weismannists is not only supported by our native Morganists, but these Morganists, in a manner unbecoming to Soviet scientists, pride themselves on the fact that their work confirms the fundamental ideas of Morgan (Dubinin, Zhebrak).

At the end of 1947, Academician Schmalhausen wrote an article for the magazine *Priroda* (*Nature*) entitled "New Features in Modern Darwinism." In content, this is an anti-Darwinist article; in it the author enumerates all our native Morganists, but does not say a word about the coryphaei of biological science Michurin, Timiryazev, Williams and Lysenko, whose works have really developed Darwinism.

It is not the conduct of Academician Schmalhausen that seems strange to us, because he could back his Morganistic conceptions only with the works of the Morganists. What does seem strange to us is that the editors of *Priroda* found it possible to publish in their magazine an anti-Darwinist article, the author of which makes no mention of the achievements of Soviet biology and boasts that the works of our native Morganists have won for themselves an honourable place in world science.

To what shore is Academician Schmalhausen looking? Why does he imagine that scientific works acquire importance only

[1] И. И. Шмальгаузен, ***Проблемы дарвинизма***, стр. 389.

when they receive recognition abroad? In this article, Academician Schmalhausen writes: "At the same time, in the U.S.S.R. the problems of the material basis and of the factors in evolution were also elaborated. Professor Y. Filipchenko and his pupils, and also Professor N. Koltsov's assistants and pupils, have studied the laws of heredity and variability with great success. An entire army of geneticists, of whom Professor A. S. Serebrovsky and Professor N. P. Dubinin deserve special mention, have successfully combined the methods of genetic analysis with the method of cytological research, and by their empirical achievements quickly won for themselves an honourable place in world science."[1]

And then follows a whole page with the names of Morganists. You will find here the names of Dubinin, Sakharov, Rapoport, Gershenson, Kirpichnikov, Naumov and Lukin, and of course, after nearly each one, the name of Schmalhausen.

Academician Schmalhausen writes: "Only here, in the Soviet Union, are anti-Darwinist trends subjected to consistent criticism," and then he gives the names of the authors who criticize anti-Darwinist trends. Who are these authors? It turns out that they are Professor I. M. Polyakov, Professor Y. I. Polyansky and again Academician I. I. Schmalhausen.

Why are our students still taught that to the Austrian monk Mendel belongs the honour of establishing the first laws of heredity (Grishko, Delone)? Not only is Mendelism-Morganism taught in our colleges and universities, but in most of them general biology and Darwinism are taught from the standpoint of Mendelism-Morganism. In a textbook on general biology, in the writing of which Professor Paramonov took part, the four authors write: "So in Darwin's time it was believed that one of the likely causes of hereditary variations was the fixation of a variation called forth by the external factor...." "But Weismann, in the eighties of the last century, criticized these conceptions that prevailed at that time." "The experimental work of Johannsen and his followers finally destroyed the grounds for such views."

In his *Textbook on Darwinism*, published in 1945, Professor Paramonov writes: "By modifications, in the broad sense of the

[1] *Природа*, 1947 г., № 12.

term, we mean non-hereditary variations due to the influence of the abiotic and biotic environmental factors. The first include: temperature, moisture, light, the chemical properties of water and the soil, and mechanically operating factors (pressure, wind, etc.); the second include: nutriment, and also the direct or indirect interaction of organisms. All these factors give rise to non-hereditary phenotypical variations of more or less profound effect."[1]

On page 380, he writes that directed hereditary variations are not observed in nature.

Grishko and Delone write that at the present time the question of the inheritance of acquired characters, or modifications, has been definitely settled in the negative.

Academician Schmalhausen totally denies the possibility of the inheritance of acquired characters and qualities, the possibility of adequate alterations in the nature of organisms in conformity with the changed qualities of the developing organism. He writes: "In the process of evolution, however, somatic mutations evidently play no part and can be left here without further examination."[2]

Academician Schmalhausen, in 1946, backed his views with references to Weismann's authoritative experiments.

He wrote: "Weismann docked the tails of mice in a number of generations. These experiments gave negative results. Certain breeds of dogs have had their tails docked from time immemorial, nevertheless, the dogs of these breeds continue to be born with tails."[3]

I. V. Michurin, the great transformer of nature, laid the foundation of the Soviet science of biology. Academician T. D. Lysenko put in an enormous amount of work in further developing the Michurin theory, which regards breed and environmental conditions as necessary sides of one and the same process of development.

According to the theory of the phasic development of plants, every preceding process creates in its course, in its development, the concrete possibility for the ensuing process of development. The processes, the organs and the characters of the

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[1] А. А. Парамонов, *Курс дарвинизма*, 1945 г., стр. 197.
[2] И. И. Шмальгаузен, *Факторы эволюции*, стр. 65.
[3] И. И. Шмальгаузен, *Проблемы дарвинизма*, стр. 188.

living organism, do not develop autonomously under the influence of genohormones; their development is mutually conditioned and interconnected—from the seeds to the formation of new seeds—in close, necessary connection with the conditions of life.

In order to change the hereditary nature of plant organisms in the course of completion of the process, conditions are created in conformity with the action of which it is necessary to change heredity.

The theory of the phasic development of plants indicates the only correct way of understanding and studying heredity. By regarding heredity as a property of the living organism which requires definite conditions for its development, Soviet, Michurin scientists really get to know whether this or that character is hereditary. Following the example of the study of winterness and springness, and also the second phase of development, Michurinists, and physiologists in particular, must, on the same plane, study the development of the organs, characters and qualities of living organisms. This will make it possible not only to direct development in the process of the individual life of organisms, but also fundamentally to change their nature in the desired direction.

On the basis of the theory of phasic development, Academician T. D. Lysenko elaborated the theory of directive alteration of the nature of organisms; this theory will enable us to control the form-building process in nature.

Directive alteration of the nature of organisms and the inheritance of acquired characters become possible because, as a rule, in the process of evolution, the organism, the process, the function and structure, are the first to change in response to changed conditions; and then, the changed organism reproduces the correspondingly changed sex cell or the vegetative progeny.

Owing to the fact that the concrete possibility of development of each process is created directly in the course of the process immediately preceding it, in the individual development, the completion of each phase, entering the next chain, determines the beginning of the very same process in the progeny.

Therefore, for directed alteration of the nature of organisms, the conditions of life under the influence of which the process is completed are of decisive importance.

The numerous spring varieties that have been obtained by directed training from winter varieties develop as spring varieties when sown in the spring; similarly, hybrids obtained by crossing winter varieties, converted into spring varieties with the initial winter varieties, develop as spring varieties. By training we have also obtained winter varieties from hereditary spring varieties. Changed organisms, not having yet acquired a stable nature, are very susceptible to the moulding conditions of life.

In this connection, the hardy winter varieties obtained in Siberia from spring varieties by means of autumn sowing are of great interest.

The rule of the dominance, the purity of the gametes, is regarded as one of the fundamental laws of Morganism. Morganism utterly repudiates the possibility of directing dominance in the first hybrid generation. From the standpoint of the Michurin theory, it is possible and necessary to direct the development of hybrid plants.

It is known that in crossing winter with spring varieties the first generation develops as a spring variety. Is it possible to conduct the work in such a way that the first generation should develop as a winter and not as a spring variety? It is. To achieve this, spring varieties, the spring variety of rye in particular, are sown before crossing in the autumn.

Spring rye plants, sown in the autumn, are repollinated in the following year with the pollen of winter varieties of rye. When we sow such hybrid grains, the progeny of every ear produces a large number of winter hybrid plants, in which winter type of development is dominant. It is very important that in the majority of cases the offspring of these hybrid plants develop according to the winter type. Only individual plants produce a small number of spring plants.

From the standpoint of Mendelism-Morganism, the nature of the divergencies in the second generation of hybrid organisms is conditioned by the chance separation of the chromosomes during the reduction division.

According to the Michurin theory, this divergence is conditioned by relatively different somatic cells from which the sex cells are formed. Hence, from the standpoint of Mendelism-Morganism, it is impossible to direct divergence in hybrid prog-

eny; from the Michurin standpoint, however, it is possible to regulate and direct the nature of the divergencies in the second and subsequent generations.

It is known that many varieties of bulbous plants, in addition to forming seeds, can produce aerial bulbs. We deliberately took hybrid plants, which, after flowering, produced both seeds and aerial bulbs. All that has to be done for this is to remove the buds before flowering. When we sow the seeds and the aerial bulbs we obtain variations in the colour of the bulb, in the form of the bulb, both in the progeny arising from the seeds and from the aerial bulbs.

These examples show that diversity in hybrid progeny is not due to the chance separation of the chromosomes during reduction division, but to the fact that in the process of the individual development of the organism the somatic cells from which the sex cells are formed change their nature.

Simultaneously with this work, we studied another problem, which in our opinion is of great theoretical interest.

It is the accepted thing in Mendelist-Morganist literature to regard the process of rejuvenation as being connected with the reduction division. We, however, assume that the process of rejuvenation should be a property of both the sex cells and the natural vegetative reproductive organs. We assume that the somatic cells from which the sex cells are formed become young. For this purpose we used bulbous plants.

It is known that winter varieties of garlic, in addition to bulbs which form at the bottom, form bulbs also on the flower-bearing shoot. If the aerial bulbs formed from cells which have passed through all stages of development are planted, the organisms obtained from these bulbs do not flower in the first year, nor in the next year if they are reared in warm temperatures. From many strains of onions we obtained aerial bulbs and seeds. It has been proved experimentally that plants obtained from seeds and from aerial bulbs require the same conditions for passing through the stages of development anew. This shows that the property of beginning individual development anew is not conditioned by the reduction division, but by definite somatic—maternal cells from which the sex cells are formed.

It is known that in practical plant breeding, in crossing distant forms, especially when different species are involved, the

hybrid plants of the first generation are sterile. Hence, from the Mendelist-Morganist point of view, it is necessary to double the number of chromosomes by means of colchicine treatment in order to obtain fertile plants.

Last year Academician T. D. Lysenko resolved to obtain a quantity of hybrid seeds from the crossing of two species of hard branched and soft wheat so as to obtain in 1948 two centners of seed from the plants of the first generation.

Last year we obtained such a quantity of seeds from the crossing of winter strains of soft wheat with branched wheat that will enable us this year to gather from the plants of the first generation of interspecific hybrids a harvest of two centners of seeds. All the hybrid plants we obtained were entirely fertile. Many of the hybrid clusters produced 1,500 grains.

Hence, it is idle to study, as the Morganists say we must do, the so-called hereditary substance in chromosomes for the purpose of obtaining desired forms from distant or not distant crossings. Practical plant breeding does not know of a single case of the production of a variety or a breed by the application of the gene theory of heredity. I do not mean to say that we need not study the chromosomes; but we must not study them as our cytogeneticists of the Mendel-Morgan trend study them.

We can also demonstrate inter-generic hybrids, wheat-couch grass (Agropyron) hybrids, by no means obtained as a result of the application of the chromosome theory of heredity. Many Soviet scientists are working to produce a perennial wheat. This is a feasible and needed task. The wheat-couch grass hybrids were fertile from the first generation. True, we have not yet obtained genuine perennial forms of wheat, but they can be obtained. For this it is necessary that the tillering should not be of the spring type, as is usually the case, but of the winter type, as in clover.

In the fourth and fifth generation, our plants began to reveal interesting properties characteristic of the rhizome forms. They develop, there is ear emergence with normal fruitful spikes, and they produce shoots of the rhizome type. In the preceding generations this property was scarcely developed, but in the subsequent generations, many of the underground shoots produce as many as six or seven underground nodules, and then

sprout as ordinary couch grass. On this basis it is possible to produce winter forms of tillers. If we succeed in doing this, we can count on such a form of wheat wintering for several years. Work in this direction is going on.

Soviet biology of the Michurin trend, the only correct science, opens wide prospects for creative work.

We Soviet scientists are imbued with but one desire, namely, to render the kolkhozes and sovkhozes the best and quickest assistance in the struggle for high yields. This is what we all wish. We can all express the conviction that we will carry out the tasks that have been set us.

Permit me to say a word or two by way of a proposal.

Voices. Please do.

A. A. Avakian. I think that on the following two questions I will express the opinion of all our Academicians. We have gathered to discuss an important question that is directly connected not only with agricultural science. Every one of us knows that the issue is the situation in the field of biology. But in the Soviet Union biology is not only a matter that concerns the members of the Academy of Agricultural Sciences. It is the science of biology in general, and undoubtedly, is directly connected with soil science, with the physiology of plants and animals, with plant breeding, with all the other branches of biology. We are of the opinion that the absence from this hall of the leading workers of many scientific institutions, and particularly the absence of the Morganist-Mendelists who are working in the Soviet Union, is unbecoming of Soviet scientists. I therefore move: that in our opinion, by this behaviour, the continuators of Weismann's teachings have placed themselves outside of Soviet science. That's the first point. (*Applause.*)

The second is: We are all pursuing the same aims and tasks. There can be no disagreements among us on that score, particularly among Academicians. For the immense work that lies before us we need unanimity, we need unity. And the common basis for this unity is our system, our Soviet system. Nevertheless, it must be said that among the members of the Academy of Agricultural Sciences there are such as have behaved in a manner unworthy of Soviet scientists. B. M. Zavadovsky, in particular, has not missed an opportunity during the past few years to vilify the Michurin trend. If B. M. Zavadovsky con-

tinues to remain absent from this session, if he keeps quiet and refuses to speak here, I think I will be expressing the opinion of all Academicians if I say that he ought to be invited to speak. (*Applause.*) And if he disregards the invitation, then we ought to demand that B. M. Zavadovsky should come here and expound his views. We ought to know whether we ought to help B. M. Zavadovsky—we must help a man who wants to adopt the Michurin standpoint, the standpoint of progressive biology. Or whether we must hinder him—if a man wants to defame the Michurin theory and to insist that Malthusianism is the cornerstone of Darwinism, we must hinder him. (*Applause.*)

Academician P. P. Lobanov. I call upon Comrade A. P. Vodkov.

A. P. Vodkov (Director of the Moscow Plant-Breeding Station). In his address, T. D. Lysenko, the President of the Lenin Academy of Agricultural Sciences, spoke of the necessity of delivering the finishing stroke to the idealistic, metaphysical trend in biology and agrobiology, and of the measures to be taken against the penetration into our science of these pernicious and, to our Soviet people, alien trends from abroad. He also indicated the direction the development of agricultural science should take and stated that the teachings of Lenin and Stalin must serve as our basis if science is to flourish.

Under our conditions, agrobiology is the scientific basis of the whole of our agriculture. This science, and those who are engaged in it, are responsible for everything that goes on in agriculture. Comrades, we must not behave like small boys in a matter like this, we cannot lark about with vast agricultural enterprises as the formal geneticists, and all those who take an irresponsible attitude towards science, do so lightheartedly.

The collectivization of agriculture was a profound revolution, equal in its consequences to the revolution in October 1917.

A new, mass form of husbandry arose—kolkhozes.

Such a form of husbandry had never before been witnessed in the history of agriculture. The old agricultural science that had taken shape under capitalism was unable to satisfy the requirements of the kolkhozes. It was necessary to elaborate a

new theory of agronomics based on the teachings of Lenin and Stalin. Comrade Stalin, the greatest scientist of our day, gave us a guiding line in this matter. At the conference of Marxist agronomists he said that we must take the theory of socialist reproduction on a progressively increasing scale as our point of departure, that if we conduct all our work from that standpoint we shall make progress.

It was necessary to elaborate such a theory of agronomics, and, in doing so, to be guided by precisely this idea. The new theory of agronomics was evolved by our Soviet scientists, Academicians T. D. Lysenko and V. R. Williams. Their works are the highest achievements of thought in the sphere of agronomics, they are an immense contribution to world science. Comrades, we must not underrate this. We need not hide our light in science under a bushel; we are working under Soviet conditions, under the guidance of Comrade Stalin, and with such innovators in science as Michurin, Williams and Lysenko.

Our kolkhozes and sovkhozes are developing on the basis of socialist reproduction on a progressively increasing scale. Soviet scientists must work out agricultural techniques that will ensure socialist reproduction on an ever increasing scale in the kolkhozes and sovkhozes, that will eliminate the element of chance from our harvests, that will improve the fertility of the soil, create conditions for steadily increasing crops from year to year, create conditions for a rapid increase in our herds and of their productivity, for the all-sided and harmonious development of all branches of husbandry, in the kolkhozes, including the secondary branches—poultry and beekeeping—not to mention those extremely important branches, horticulture and vegetable growing.

In the works of Academician T. D. Lysenko and V. R. Williams we see the masterly application of the laws of Lenin and Stalin to the science and industry of agriculture. Lysenko and Williams took the best from their predecessors, creatively revised the science of agronomics, developed it further and elaborated a new one of their own, which helps us to carry out the tasks which our Party and Comrade Stalin have set agricultural science and production.

Under the influence of the wonderful ideas and splendid works of Academicians Lysenko and Williams, work is pro-

ceeding on an ever growing scale in the Kamennaya Steppe and at the Moscow State Plant-Breeding Station. The guiding line in this work is the proposition advanced by Academician Lysenko, that the organism builds itself up out of nutriment, and changes in conformity with the influence of environmental conditions, and also the utterances of Academician Williams who formulated the question as follows:

"Shall we strive to improve our plants so as to *obtain an organism capable of developing the utmost productivity under the best conditions*? Or, on the contrary, admitting our utter impotence to create favourable conditions for the work of plants, shall we strive to produce a plant that will be able to work under the worst conditions, or be able to utilize the ephemeral conditions of anarchic husbandry? . . .

"I cannot conceive of any hesitation in the choice of our direction. On the one hand, there is the wide prospect of creating conditions for the most perfect and productive plant organisms. On the other hand, there would be hopeless belief that existing conditions are insurmountable and resignation to the minimum of productivity in agriculture."

Each one of us is constantly faced with the task of creating the necessary environment for agricultural plants, animals and microorganisms. The breeder who fails to know how to create this environment for plants and animals, who does not understand the laws of agriculture, is good for nothing. He is blind.

But what is environment? In the works of Trofim Denisovich Lysenko, and in the works of Vasily Robertovich Williams, this environment is presented as a most intricate and most interesting complex. We create the environment for plants, but at the same time, these very plants serve as the environment for animals and microorganisms. And animals and microorganisms, in their turn, serve as the environment for plants. This is what the minor biological cycle of substances in nature consists in. We must master it in order to regulate the formation of the organic substances that are created in the soil only by green plants, and which are indispensable and most valuable materials for agricultural production.

We are faced with the task of conducting agriculture in such a way as to make it easy to regulate the interconnection, the interrelations and interdependence of the development of

plants, animals and microorganisms. They must develop for the benefit of man, ensure continuously improving conditions for each other by creating the largest quantity of organic substances and that part which is of greatest value for agriculture. This is what our Academicians must work on. It is in this that we rank-and-file scientific workers must help the Academicians.

All this will be successfully achieved if we apply the teachings of Lysenko and Williams concerning the influence of environment on the organism, and concerning the travopolye system of agriculture; if, I want to emphasize, the latter is included in the crop rotation system. It is not enough to say lea rotation—rotation with perennial grasses, rotation with legume and cereal grass mixtures. This indicates only one aspect of the matter. We must create environmental conditions for all the organisms with which agriculture has to deal. We will be able to solve this problem only within a combined system of fodder and field crop rotation, which must be introduced in every kolkhoz.

The situation is still unfavourable in this respect, however. Agrobiological science still takes too small a part in the work of introducing crop rotations. Arithmetic plays the chief part in this. We must give things such a turn that agrobiological science shall serve as the basis of the crop rotation system. It must give the direction, it must serve as the point of departure.

The work of the Kamennaya Steppe Plant-Breeding Station has brilliantly proved that the crop rotation system, and the complex execution of all production tasks, result in a steady increase in crop yields from year to year. Before the travopolye system was introduced, the Kamennaya Steppe Station obtained cereal yields of 6.7-10.1 centners per hectare; but when the travopolye system of agriculture began to have effect, yields steadily increased until they reached 24 centners per hectare.

Following the example of the Kamennaya Steppe Station, the Moscow State Plant-Breeding Station introduced different systems of crop rotation with the view to creating conditions for plant breeding and seed cultivation. First of all we worked out a method of arranging crop rotations and then we applied this method at our station and at a number of kolkhozes. I will not go into the details of the results we obtained, I will merely say that thanks to the fact that we had correctly worked out

the proportions of areas for fodder and field crop rotations, wide prospects were opened for farming. First, it created the possibility of bringing under cultivation all arable land that has been allowed to remain wild up till now. Secondly, it has become possible at the station to increase the woodland area by 390 hectares and to work out a shelter-belt system.

Thanks to our having mastered the elements of the travopolye system, we are already getting larger yields at the Moscow Plant-Breeding Station, larger areas are being planted to wheat and industrial crops, and the total crop is increasing. The milk yield of cows has greatly increased. Stockbreeding is expanding. Our station now has the opportunity of enlarging its herd of cattle from 360 to 1,490 head.

At the Novy Put Kolkhoz, in the Tula Region, where we are working, the herd is to be increased 62%; at the Svobodni Kolkhoz, in the Tula Region, it is to be increased 227%, and at the Krasny Put Kolkhoz by 160%.

At the Osoaviakhim Kolkhoz, Moscow Region, which is our base, the herd is to be increased from 194 to 311 head. This is the initial herd; in future it will grow still more, as there will be an increase in the cereal and fodder crops.

The lea crop rotation system creates conditions for a continuous expansion of stockbreeding and improvement of agriculture. We have concrete evidence of how one branch stimulates the growth of others, and the best conditions are created for the development of all branches of husbandry.

Agronomists, scientific researchers, and agrobiological science as a whole is faced with the task of teaching workers how, practically, to organize their territory on the basis of agrobiological science and not only on arithmetic. We must work out crop rotation systems for definite kolkhozes and ensure the best conditions of life on that territory for all organisms—plants, animals and microorganisms. We must establish such an interaction of agricultural organisms as will least hinder their interaction and allow for the utmost support and stimulus to growth of one branch of husbandry by another. That is why plant breeding, animal breeding and field cultivation are, as Williams always emphasized, equally important branches of agriculture. They must be looked at from the point of view of their interconnection. Otherwise, things won't work well. We

must abolish the gaps and lack of coordination between the different branches. This is the mission of science.

In the light of the teachings of Lysenko and Williams, the problem of fertilizers now assumes a new aspect. This is the latest that occurred in our Serebryanniye Prudy District.

By order of the Moscow Regional Agricultural Administration 720 tons of potassium fertilizer were sent to the district. But experiments made at a number of kolkhozes, and at our station, showed that at the present stage, potassium is not only of no benefit to cereal crops, but may even be harmful. The indiscriminate planning of fertilizers proved to be a mistake, and the Moscow Regional Agricultural Administration was obliged to rectify its mistake. Fertilizers must be put on the fields in a differential manner, strictly in accordance with the crop rotation and the crop. I will quote an example. At the Klemovo branch of our station, nitrate fertilizer is extremely effective, all others are useless for the time being. At another branch—Novaya Usadba—phosphorite fertilizers are beginning to be useful, in addition to nitrates. In the fields of the Myagkovskoye branch of our station, fertilizers have to be employed in doses other than those that are effective at the first two branches.

From this it follows that every farm and every field, at a definite stage of the general process of agriculture, requires different kinds and different doses of fertilizers.

A voice. What results did granulated fertilizers show at your station?

A. P. Vodkov. We tested granulated fertilizers in production and obtained splendid results.

At the Moscow Plant-Breeding Station we have worked out a method of determining the fertilizer system. We lay out an experimental plot for fertilizers for a definite crop in each crop rotation field in the year before the particular crop is sown. For example, if this year a given field is sown with spring wheat and next year is to be sown with legumes, a hectare is left in the middle of the wheat field and sown with legumes. This enables us to judge how much fertilizer the legumes in this field will require, and what kind of fertilizer to get ready for it. True, next year will not be the same as this year, but with the travopolye system of cultivation, the influence of the weather is greatly reduced.

The question of varieties also presents itself in a new light under the travopolye system of cultivation. It is known that in the Kamennaya Steppe, spring wheat was formerly regarded not as a crop, but as a curse. Today, spring wheat is a boon and gives high yields under the travopolye system of cultivation. At the Moscow Plant-Breeding Station too spring wheat was regarded as a risky crop, but practice has shown that under the travopolye system of cultivation it is possible here too to obtain steadily increasing crops year after year.

Thus, last year we harvested 12.6 centners per hectare from 180 hectares, and this year the spring wheat yield is still higher.

In the zone in which our station is situated, alfalfa is a necessary grass; without it there can be no true crop rotation. It turned out that alfalfa can be successfully cultivated in our parts. Bacterization of the alfalfa seeds before sowing increases the grass crop by 50% and more. Up till now alfalfa has produced little seed under the conditions prevailing in our district. Experiment showed that seed could be obtained if alfalfa is sown in soils rich in organic matter. Research revealed a fact that should interest biologists. A monolith dug up from a depth of 70 cm., from a plot which had been manured with feces, revealed not a single tuber at the alfalfa roots, although the seed crop from this plot amounted to as much as 4 centners per hectare. But alfalfa taken from a plot that was poor in organic matter had an extraordinary number of tubers at the roots, but there was scarcely any seed crop. This is one of the important problems that ought to be studied.

The situation in agriculture urgently brings up the question of the type of scientific research institutions we need. I think that the work of plant-breeding stations, and of the Kamennaya Steppe and Moscow stations in particular, shows that we must endeavour to set up scientific research institutions of their type.

They should be scientific-production institutions, and not simply scientific institutions. On this question Academician Williams wrote that experimental stations should be model sovkhozes with all branches of husbandry—stockbreeding, a crop rotation system, and modern machines.

The methods of scientific work employed by T. D. Lysenko are amazing. He conducts researches at kolkhozes and sovkhozes on areas amounting to hundreds of thousands and even

millions of hectares. This form of organization of scientific work safeguards the scientific worker from scholasticism in his research work.

The Moscow State Plant-Breeding Station is also proceeding along these lines. I think that in experimental institutions in which all thoughts are concentrated on the task of obtaining the largest possible agricultural output, on obtaining bumper crops, advanced science of agronomics will flourish. (*Applause.*)

Academician P. P. Lobanov. I call upon Professor Z. Y. Beletsky.

Professor Z. Y. Beletsky (Head of the Department of Philosophy, Moscow State University). This discussion of Academician T. D. Lysenko's address is an event of great moment. We are here summing up the dispute which has gone on for many years between the two trends in biology—the formal geneticists on the one hand and the Michurinists on the other.

The events taking place in the sphere of biology are to a large extent analogous to the events which took place in the sphere of philosophy. Both in philosophy and in biology we have to deal with phenomena of the same order. A struggle is going on between two trends: the bourgeois, idealist trend and our dialectical materialist trend.

The representatives of the Weismannist trend not only defend a bourgeois theory in biology; they also insinuate the pernicious idea that bourgeois science and Soviet science are identical.

We find that some people among us accept the bourgeois view that our Marxist world outlook, our theory did not spring from the conditions of the new social and material relationships among men, but is the result of a generalization of all preceding intellectual achievements. According to this bourgeois conception, science sets itself the abstract task of comprehending the phenomena of the world in general. This bourgeois conception has it that the various sciences have developed outside of political life, outside the class struggle. From this the conclusion is drawn that only he is a true scientist and really advances the life of society who has mastered all the achievements of bourgeois theory, past and present. There is only one

thing that should interest such a scientist, to wit, how ideas are connected with ideas and how they flow from one another. Such a scientist can therefore carry on his work in the quiet of his study. He is a "high priest" of science. He need not worry whether his speculative schemata accord with life or not. The important thing is the theory, not life, not practical activity. The motto of such scientists is: Let life adjust itself to science; if it is unable to do so, so much the worse for life.

That is why our Soviet Weismannists—Schmalhausen, Yudintsev, Alikhanian, Zhebrak, and the others—who assimilated the wisdom of Morganist-Mendelist genetics, have decided that they alone are the genuine scientists, and that our Soviet practice must endeavour to keep abreast of them. And if practice does not bear out their theories, so much the worse for practice. That is why for many years they looked down upon and made light of the practical successes of Michurin biology. That is why the Faculty of Biology at the University of Moscow—a stronghold of Morganist-Mendelist reactionary genetics in our country—has been fighting tooth and nail against the new genuinely scientific biology created by I. V. Michurin and so splendidly carried forward and developed in our day by T. D. Lysenko.

I shall cite a few facts to give you an idea of the doings of the Weismannists in the Faculty of Biology at the Moscow University.

In the past decade the Faculty of Biology has systematically held meetings, academic sessions and conferences devoted to criticism of Academician Lysenko's theoretical views.

Now, don't think that it was anything in the nature of serious scientific criticism. Not at all. Academician Lysenko's views were rejected from the outset as betraying ignorance, as having nothing in common with "genuine" university learning. That is the opinion of Michurin's and Lysenko's teachings held by the majority of the professors and instructors on the Faculty of Biology, and that is also the opinion inculcated upon the students in that Faculty. Here is an instance. Last February the Faculty held an all-Union scientific conference, which lasted a week. About forty papers were read. But what were the problems the conference discussed? Did it discuss the achievements of biological science in practical farming, or did it demonstrate

the advantages of our biological science as compared with bourgeois science? It did not. Every paper read at the conference, from the first to the last, was an attack on Academician Lysenko's teachings and a defence of bourgeois genetics. It appears that the main task the scientists on the Faculty of Biology set before biological sciences in this year 1948 is to refute the teachings of Academician Lysenko.

How far the heads of the Faculty of Biology have gone in prosecuting this task may be gauged from the methods they employ. Here are a few instances.

After the *Literaturnaya Gazeta* printed the interview with T. D. Lysenko on the subject of intraspecific struggle, the Council of the Faculty of Biology held a meeting at which Academician Lysenko's view was subjected to sharp criticism. After that meeting the Department of Dialectical and Historical Materialism of the Moscow University called a meeting of its own to discuss the same subject.

Now what was the reaction of the heads of the Faculty of Biology to this meeting? The beginning was made by the Department of Darwinism. It demanded that a joint meeting should be held. Why? The explanation given was that the Department of Darwinism feared that the Department of Dialectical and Historical Materialism could not itself handle the problem intelligently enough. When the representative of the Department of Darwinism was told that the latter's view on the matter was known, and that the Department of Dialectical and Historical Materialism wanted to have an independent discussion of its own, the representative of the Department of Darwinism, the spokesman of the biologists, said: "If you give your support to Academician Lysenko you shall answer for it and take the consequences. The University must be united in its opinion."

The Department refused to obey. It violated the University's unity. It expressed its viewpoint in the *Literaturnaya Gazeta* and in the University newspaper, *Moskovsky Universitet.* The consequences of this step made themselves felt soon enough. The warning given by the Department of Darwinism was put into effect. The leadership of the Faculty of Biology now proceeded to demand that not only Academician Lysenko's teachings should be kept out of the University, but that there should

be a change of personnel in the Department of Dialectical and Historical Materialism. Further developments took the following course.

At an important meeting of the University the Head of the Genetics Department, Docent Alikhanian, made a statement on behalf of the Faculty of Biology. In view of the fact, he said, that the Department of Dialectical and Historical Materialism had proved unequal to its tasks in the sphere of biology, having shown how little it was versed in theory, he considered it necessary to suggest a renewal of personnel. By all appearances, the suggestion was taken into consideration. The Rector immediately appointed a committee to investigate the Faculty of Philosophy. The committee worked for two months, after which it reported to the Council of the University. On the basis of the report the Council adopted a decision in line with Docent Alikhanian's demands. The decision was not put into effect, but only owing to circumstances beyond the control either of Alikhanian or the committee.

What was the attitude of the heads of the Faculty of Biology during the period the Rectorate was preparing to carry out measures for the "renewal" of the Department of Dialectical and Historical Materialism? The Faculty of Biology applied the tactic of sabotaging the Department's work. The Dean of the Faculty, S. D. Yudintsev, made it impossible for the seminar of the course in dialectical and historical materialism to go on with its studies. He demanded that the Department should supply another instructor to replace Comrade Furman, because the latter was an open adherent of Academician Lysenko's teachings. The Department refused to comply, and the result was that for a whole semester there was no seminar.

A few words about the students of the Faculty of Biology. The methods of intimidation employed in regard to them are simply incredible. Students of the Faculty of Biology are emphatically told that they must criticize Michurin's and Lysenko's teachings. If individual students nevertheless disagree with the Weismannists, they dare not say so openly. Some of these students, when they come to the Department of Dialectical and Historical Materialism for consultation, insistently request that their names and opinions should not be divulged.

The heads of the Faculty of Biology are doing their best

to eradicate Michurin's and Lysenko's views not only from the minds of the students, but from the minds of the professors as well. In 1944 Academician Schmalhausen's work, *Problems of Darwinism*, was sent to me for comment. In that work Academician Schmalhausen described in brief Michurin's and Lysenko's experiments, and in general spoke approvingly of their scientific achievements. I was heartened by that fact, because, as I have said before, in the Moscow University the names of Michurin and Lysenko were only being mentioned as synonyms of ignorance and narrow practicality. I don't know why Academician Schmalhausen then wrote about Michurin and Lysenko the way he did. Perhaps it was due to an oversight on the part of the Dean of the Faculty, Comrade Yudintsev; perhaps the reason was that Docent Alikhanian was away from Moscow at the time. My comment was favourable, although I at the same time pointed out the major theoretical defects of Schmalhausen's work. The upshot of this is that now there is hardly a meeting held but Docent Alikhanian or somebody else gets up to make a statement objecting to that comment: How, they demand, could Professor Beletsky assume that Academician Schmalhausen could express a favourable opinion of Michurin's and Lysenko's views, when it is known to everyone that Schmalhausen shares the viewpoint of Morgan-Mendel genetics? As for Academician Schmalhausen, he hastened to "atone" for the sin of his youth: He has written a new work, *Factors of Evolution*, in which Michurin and Lysenko are not mentioned at all.

You can see from what I have said how persistently the heads of the Faculty of Biology at the Moscow University have fought against Michurin's and Lysenko's teachings. It is hard to understand why Yudintsev, Alikhanian and the others are now keeping silence. Is it because they have nothing to say, or because they think that this is just one of the ordinary discussions which does not concern them and is no concern of theirs? Apparently, they believe that they don't need to say anything now because they will have their chance to get even at a conference of their own in the University.

But we may assume that their hopes will not come true. Our Party is so strong because it knows what it is fighting for, and in the name of what ideas, what theory, it is achiev-

ing its victories. The teaching of I. V. Michurin and T. D. Lysenko has been tested by the practice of socialist construction. The theoretical foundation of this teaching is dialectical materialism. It is to this teaching that the future belongs. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician E. I. Ushakova.

Academician E. I. Ushakova. Comrade Beletsky, Head of the Philosophy Department at the Moscow University, gave us a good picture of the situation in the University in regard to Michurin genetics. I was greatly surprised to hear that in a Soviet university there peacefully coexists, along with a Philosophy Department which is supposed to teach the Marxist-Leninist philosophy, a different department which, contrary to the Marxist-Leninist philosophy, upholds idealist, reactionary currents.

Comrade Beletsky remarked in passing that they had taken a tolerant attitude to these harmful currents in biological science (Morganism-Mendelism). But of our philosophers we expect a much more militant materialism than of others, and it is absolutely incomprehensible how they could reconcile themselves to such facts as have been cited here.

From Comrade Beletsky's account it appears that the genetics studied in our universities and in some institutes is made to look like a highly exalted science, one that calls itself and considers itself a truly theoretical science, standing above the science which is being created by practical workers and investigators.

We highly esteem every science, but only so long as we feel that it opens up vistas before us. But what has the science which we call Morgan-Mendel genetics given us—what promises has it made and is still making? It once promised us many things, but has given us nothing. Today this "science" is again making many promises, but it can hardly give anything.

The Gribovo plant breeders have always regarded themselves as very modest practical workers. They have studied the initial material and have worked to improve old and produce new varieties. The results of our station's work are known far and wide and have been applied throughout the country. But there

were years when the "exalted" science of the Morganist-Mendelists made our workers turn from the straight road, because that exalted science seemed to be offering some prospects. However, two-three years of work in accordance with the methods of Morgan-Mendel genetics revealed their harmfulness.

Inbreeding, which was introduced with the aim of obtaining a rapid morphological uniformity of varieties, immediately revealed its extremely pernicious results: there was a sharp decline in yield and a lowering of resistance of the plants to unfavourable conditions of the environment. As soon as the Gribovo plant breeders became aware of this defect, they abandoned that doctrine and set to work the way they considered right, the way Michurin teaches us.

In my case it so happened that I first studied Darwin, Timiryazev, Williams and Michurin, and became acquainted with the Morganist-Mendelist theory later. The impression I received when I familiarized myself with this theory was the same as when I studied idealist philosophical conceptions. In these conceptions everything is topsy-turvy. Just as it is hard for a normal person to understand and accept the notion that the world we see is not an objective reality existing outside of us, but only our idea of it, only the result of our perception, so it is hard to understand how the Morgan-Mendel theories can be applied in practice.

I confess that at one time I devoted a great deal of effort to understand how people came to create idealist conceptions, how such "theories" could be conceived. In the end I realized one thing, namely, that the authors of idealist conceptions apparently never created anything themselves, but only indulged in speculative theorizing. The Morganist-Mendelists produce the same impression. They have never set themselves the aim to create new varieties of plants and new breeds of animals, but only studied the "mechanism" of heredity, and this has led them far away from true science, from the science needed for practical activity.

"Exalted science" often trammeled what the breeders were doing. It was not for nothing that there existed two terms: "theoretical breeding" and "practical breeding." Practical breeding was regarded as something lowly, something that everyone could do—select, plant, grow seeds. Theoretical breed-

ing was elaborating "lofty principles" of improving breeds and producing new varieties; by theoretical breeding was meant the theory of the Morganist-Mendelists.

Comrade Stalin says that theory becomes aimless if it is not connected with revolutionary practice, just as practice gropes in the dark if its path is not illuminated by revolutionary theory. The revolutionary theory is the theory which changes the world, which rebuilds it.

But what does the genetics of the Morganist-Mendelists represent in this respect? How much, indeed, the Morganist-Mendelists have fallen behind revolutionary Soviet practice in the matter of creating new plant varieties and new breeds of domestic animals! Thanks to the creation of new varieties of plants, life has invaded the Arctic regions and the deserts, our country is becoming a flowering garden. While the Morganists have been labouring over the *Drosophila*, the remarkable Kostroma cow has been produced, a cow with an exceptionally high milk yield reaching 14,000 litres per year, new breeds of sheep and hundreds of new plant varieties have been created. But where is the contribution of the Mendelist-Morganists to practical socialist agriculture? Perhaps they are still busy accumulating strength? But if that is so they have been taking too much time for it. It is high time they stopped accumulating strength and gave their achievements for the common good.

Many of you must remember the years 1934, 1935, 1936 and 1938, when the controversy over fundamental questions of Darwinism raised by Academician Lysenko was at its fiercest. In those years the position of the opponents seemed to be quite strong, their adherents were rather numerous, and they joined forces to give battle to creative Darwinism, the idea and study of which was then put on the order of the day by T. D. Lysenko. They went to the length of bandying unworthy expressions and slanders, browbeating young scientists, warning them that world science would not tolerate Lysenko's denial of the gene as the bearer of the substance of heredity. They had no other name for those who shared Lysenko's teaching and theories than ignoramuses and half-baked scholars. These words have always been used by obscurantists in their effort to smother everything that is fresh and creative. Practice has shown, however, on whose side the truth of life was, who was backed by

the revolutionary theory. But, although a relatively long period of time has elapsed since those years, the propaganda of obscurantism is still going on in the genetics departments of our institutions of higher learning. Propaganda of obscurantism—there is no other name for it.

Voice. Hear, hear!

E. I. Ushakova. Who has given these "scientists" and "teachers" who call themselves Soviet people the right to corrupt and poison the minds and souls of our young specialists! We meet scores and hundreds of young graduates of the Timiryazev Academy. When they come to us, we find that they are opposed to Michurin's teachings. How can they be good agricultural workers, if their attitude to living organisms is based on idealist notions? Comrade Beletsky, too, has told us about the efforts to muddle the minds of students at the University, where some of them are up in arms against the materialist philosophy because this philosophy does not accept the Morganist genetics. It is an intolerable situation that he has described. Students, our future Soviet experts, are being brought up ideologically in a spirit which is alien to Soviet society, to our science and practice! How could things be allowed to come to such a pass? Is it not time somebody was seriously called to account for this?

Our universities and colleges teach the History of the Communist Party, a course in Leninism; but alongside of that they teach Morganist genetics! This means an utter disparagement of the achievements of our great scientists, the Darwinists—not only a disparagement of their achievements, but an attempt to discredit them. Comrade Avakian was quite right in suggesting here that it was time to put an end to this and to recommend to our adversaries, if they have no intention of mastering the Marxist theory and the materialist philosophy, that they should keep at a distance from science.

The Morganist-Mendelist theory has proved a complete failure. But it has caused a great deal of harm. Here is one fact. In 1935, when tons of seeds of the White Icicle radish were grown in the Krasnodar Territory, it transpired that the radishes ran to seed before they formed edible roots. Many scientists and institutions got interested in this question. The plant breeder Agapov made some investigations into the matter and established that the cultivated radish may thus run to

seed when crossed with the wild variety. Having established this "defect," Agapov suggested the following method of avoiding it in seed growing: Radish should be planted as a pricked-off crop and free of hybrids with the wild radish, which begins to shoot up and flower early. What were the consequences? When the hybrid produced by a cross between cultivated radish and the wild variety gets into seed radish (which is ascertained by the method of ground control) the seeds are rejected and not permitted for use in seed growing. But since the Gribovo Station is located in a zone in which the wild radish is widespread it is impossible to avoid an admixture (even if only in fractions of one per cent) of hybrid seeds to the élite seeds grown here. And when there is no more than 0.2-0.3% of hybrid seeds, the whole lot, even if of the best quality, is condemned as unsuitable for seed growing. Those who insist on rejecting such seeds are convinced that even if the radish root grown from them does not run to seed prematurely, it all the same carried within it (in a latent form) the properties of the wild radish, and that even if the crop is cleansed of hybrids, which are distinguishable by external characters, hybrid plants are bound to grow again from the normal roots. Agapov went out of his way in this matter. Previously this hybrid had been regarded as a wild radish and removed along with the weeds. Now we are suffering from Agapov's discovery.

The Morganist-Mendelists and their followers intimidate young scientific workers who believe in Michurin genetics. I know of several cases of theses submitted in the Timiryazev Agricultural Academy, when no official opponents and no reviewers could be found. That was the case, for example, with Comrade Alexeyeva, whose thesis dealt with the question of vegetative hybridization, and with Comrade Yurina, whose thesis remained pigeonholed for a whole year only because it was entitled: "Elaboration of Methods of Vegetative Hybridization in the Cucurbitaceae." A whole year Comrade Yurina looked for a reviewer, but no one wanted to tackle the job. In the end the suggestion was made to her: "Change the title. Why call it, 'Vegetative Hybridization'? Better use the term 'Study of Graftings.'" I myself had a similar experience, because in my thesis I analyzed the behaviour of vegetables planted in the late autumn from the standpoint of the theory of phasic

development. No reviewer could be found in the course of half a year. I am grateful to J. H. Eichfeld, who came to the rescue and gave a review of my work. That is the situation everywhere. Master's or even Doctor's degrees are readily conferred on persons whose biological investigations fit in with the Mendelist-Morganist conceptions, but it is very hard to obtain a hearing for works based on creative Darwinism, on Michurin genetics. I think it is time an end was put to this state of affairs.

I shall now dwell on a few investigations of the Gribovo Breeding Station. Ever since the breeders at the Station discarded some of their erroneous methods, such as inbreeding, the productivity of the work has greatly increased. A large number of new varieties have been created and are now successfully grown on farms. Here are a few examples of hów we surmounted what "faithful" geneticists asserted was hard to surmount.

In the period between 1932 and 1937 A. V. Alpatyev produced a new type of tomato, an erect, early maturing, high yield variety with good fruits. (*Demonstrates several fruits.*) Were there such forms before? No, there were no such forms. As a matter of fact, it was asserted that it is in general impossible to create erect tomato varieties which would be early maturing and yield large fruits. Here is the initial parent form (*demonstrates a fruit*): it is very late maturing and of small size. At that time there were no other, better varieties. Thus, the above-mentioned assertion of the Morganist-Mendelists has been proved by the Michurin methods of breeding to be unsound. Do we need an early maturing form of erect tomatoes? Of course we do, because, unlike existing forms which branch out, form a large number of offshoots and drop unless tied or otherwise supported, it makes it possible to cultivate the soil practically throughout the vegetation period, requires no expenditure of labour on off-shoot pruning, pinching off, tying the stems; at the same time it has a high yield, and sets fruit in abundance even in years not very favourable (with temperatures lower than usual).

The Ilyich Kolkhoz in the Kuntsevo District, Moscow Region, last year obtained 65 tons of tomatoes per hectare.

The tomato produced by A. V. Alpatyev is the first of its type. There followed other early maturing strains of the same

type, but with larger fruits—for example, the Planovy tomato, which, though it matures a little later, has large fruits of excellent flavour and gives a high yield.

Nor was that all. It was still necessary to produce early maturing varieties with larger fruits, and now A. V. Alpatyev already has a number of hybrids as early maturing as the first variety he produced, but with larger fruits. Here is one of the new varieties (*demonstrates a fruit*)—it is an early maturing large-fruited variety. It is called Shtambovy Krupnoplodny. It gives excellent fruits of high quality and very early maturing.

All these, however, are pricked-out tomatoes. For a wider extension of the tomato culture we must produce early maturing varieties not only for pricking out, but such as can be planted directly in the open or in seedling beds, varieties resistant to lowered temperatures in spring and setting fruit in wet years with lowered temperatures which are frequent in our parts.

This was the problem we faced not so long ago, and now it has already been solved by A. V. Alpatyev who has produced a number of varieties of outdoor tomatoes. Here is one of their parents (*demonstrates several fruits*): it is of the Best-of-All variety, grown from seed planted out-of-doors—a very strong plant, with fruits on it, though they are just beginning to set. Here is the Gribovsky Gruntovoy variety (*demonstrates fruit*), one of whose parents is Best-of-All. The seed was planted directly out-of-doors on May 8; we now have mature green fruits, and in a few days the crop will be gathered in. The yield reaches 8 kg. per shrub and 140 tons per ha. The new variety of outdoor tomato endures, according to our observations, frosts of 2-3° C without any injury. Many experimenters have written to tell us that these tomatoes stand frosts of 5-6-7° C.

Here is another variety of outdoor tomato (*demonstrates sample*)—Gruntovaya Skorospelka (Early Maturing). Its yield is just as large and its fruits as good. There are out-of-doors varieties with still larger fruits, but they mature just a little later than these two varieties.

How were they bred? Young hybrid seedlings were planted directly out-of-doors in the beginning of May. A strict culling was then made for sturdiness, earliness, yield and quality of fruits.

The out-of-doors tomatoes have also proved to be the ear-

liest maturing in planting-out culture, ripening 8-10 days earlier than, for example, the Bison tomato (our earliest variety). They are now used by kolkhozes not only in out-of-doors but also in pricking-out culture. And we are not going to stop there.

Having set out to introduce southern plants in the neighbourhood of Moscow, we have continued farther along the same lines and followed up our work on tomatoes with eggplant and peppers. Here is a shrub of the eggplant Ranny Karlikovy (Early Dwarf). The fruits were technically ripe about twenty days ago, approximately on July 10. The yield, even if we get only one fruit from each plant, will reach 15 tons per hectare; but there are seven and eight fruits on a plant. It is the same with peppers. (*Demonstrates samples.*) Here is one variety—Ranny Krugly (Early Round). The fruit was technically ready long ago; this now is the seed maturity. A very pretty, round fruit, but of small size. Here is a somewhat later maturing variety, but the fruit is larger. It is the Otborny Severny (Select Northern). Technical maturity was reached ten days ago. Southern crops are thus not an unattainable dream to us.

Here is another example showing that some "stable correlations" have been overcome. It was believed that the large seed of the sugar pea is connected with the long stem, and that it is impossible to destroy this correlation. But here is a new type of plant (*demonstrates sample*)—a sugar pea, semidwarf or almost dwarf, with huge seeds. So the mentioned correlation no longer exists as something insurmountable.

Lastly, there is our work for the last nine years to breed melons to be grown in the Moscow Region. We set ourselves the aim to produce melons which could be grown at any kolkhoz and sovkhoz. Generally, there is nothing unusual about the growing of melons. They can be grown, for example, from hothouse sprouts later planted in seedbeds, with subsequent protection from cold, and also by various other methods. But the cultivation of melons can only become widespread if it is made still simpler. I do not thereby exclude a high level of agrotechny. I only have in mind smaller expenditure of labour.

By employing the complex method of vegetative and sexual hybridization and, at the same time, rearing hybrids in out-of-doors conditions, we have obtained melons which now

grow well in the Moscow Region. In any event, this year dozens of collective farms are growing the Gribovskaya Gruntovaya melon—some by planting the seed directly out-of-doors and others by pricking out. There have been few cases of unsuccessful culture. In the case of pricking-out culture the Gribovskaya Gruntovaya melon begins to ripen on July 1, and the collective farmers say that the fruits are of a good quality, although it is a population variety which yields, along with excellent fruits, fruits that are unpalatable but nice in appearance. We are not yet through with our work on this melon, but it is already clear that it behaves better than the melons brought from the South.

Here is another variety—Gribovskaya 13. It matures later and has a longer vegetation period than the Gribovskaya Gruntovaya melon. The Gribovskaya 13 melon, sown on April 15 in greenhouses or hotbeds, was planted out-of-doors on May 20 and ripened on July 14. It is thus a very early maturing melon. It is remarkable that it suffers from no disease, is very resistant and hardy. (*Demonstrates sample.*) Here a plant form has been created which is not found in nature. So much for the melon. And here is a watermelon: The fruit set after the 4th leaf, whereas in the case of even the earliest maturing varieties the fruit sets after the 8th, 9th, or 12th leaf and in the case of the later maturing varieties the fruit sets after the 22nd or the 24th leaf. We have plants on which the fruit set after the 2nd leaf. Thus the earliness which is indispensable in our conditions is being induced. Here is a plant sown on June 2: the fruit is small (there are much larger fruits too); such a fruit will be fully ripe by the end of August, like the fruit of the melon.

The earliness of the Gribovskaya Gruntovaya melon is also explained by the fact that an entirely new form has been created. Here is a primary stem on which the fruit set after the 1st leaf: its dry seeds were planted out-of-doors on the 2nd of June, ripening will begin in ten days. Last year, between August 15 and August 31, we obtained two tons of ripe fruits from 0.1 hectare by ordinary cultivation, without special heating of the soil, and with only 50 tons of manure to a hectare ploughed up with the soil, as is usually done in the cultivation of cucumbers.

I could cite many other investigations directly embodying

the behests of I. V. Michurin on the production of new varieties of agricultural plants.

The experience of many research institutions, in which new varieties of plants and animals are being created, confirms that the only effective, revolutionary, viable theory is that of Michurin, Timiryazev, Lysenko. This theory, for whose further development T. D. Lysenko is fighting so vigorously, has been tested in practice and is the only right road for the development of Soviet biological science.

Everything else must be swept away from our road, as harmful and impeding our progress. (*Applause.*)

Academician P. P. Lobanov. I call upon G. P. Vysokos.

G. P. Vysokos (Director of the Siberian Scientific Research Institute of Grain Husbandry). In the past century, ever since a beginning was made to bring the vast expanses of the Siberian plains under cultivation, settlers from the Ukraine and Central Russia brought with them winter-wheat seeds. The first attempts to grow winter wheat in Siberia invariably ended in failure. Sown on summer fallow, the wheat perished from the severe frosts. But the Siberian farmers never gave up the idea of growing high-yielding winter wheat in their fields.

It is hard and unprofitable for collective farms and state farms to carry on large-scale grain farming with spring crops alone. This is all the more inexpedient in Siberia, where the summers are short and the harvesting season for cereals is very limited. There can be no doubt that a stable winter wheat crop in Siberia is bound to lead to a large increase in the productivity of labour, making it possible to use tractors and other machines more efficiently and, hence, ensuring the further progress of grain growing on collective farms and state farms.

That is why it is a task of great moment and urgency for Soviet agrobiological science and for the large army of collective-farm Michurinist experimenters to solve the problem of winter wheat for Siberia.

Until recently science was baffled by this difficult problem. Trial sowings of winter wheat on fallow as a rule proved unsuccessful, as the seed perished in the winter. Academician T. D. Lysenko explains the reason why winter wheat

sown on fallow in Siberia does not survive the winter; it is due, he says, to mechanical injuries suffered by the underground parts of the plants and their leaves. Observations made by our Institute in the course of six years on winter wheat sown on fallow have fully borne out this explanation. When there is no snow cover from the fall, the soil freezes all through and there appear numerous deep cracks in it, and it becomes considerably deformed. That is the cause of mechanical injuries to the tillering nodes and the roots of winter wheat. Cold winds carrying numerous sand particles damage and sometimes completely destroy the leaves of winter wheat in the autumn and winter. Even the most winter-hardy varieties of winter wheat, such as Lutescens 329, cannot withstand the severe Siberian winter when sown on fallow. The conditions on fallow fields have proved to be absolutely incompatible with the biological requirements and possibilities of winter wheat plants.

The first Siberian agronomists Shcherbakov and Obukhov, who worked on an experiment field of the Siberian Cossack Voisko near the city of Omsk, attempted as far back as in the thirties of the past century to secure by various methods of snow retention the overwintering of winter wheat in fallow. After them, in the course of a century, many scientists and practical farmers tried various methods of sowing winter wheat on bare fallow and on fallow with snow retention, i. e., they tried to grow wheat in Siberia the same way it is grown in the Ukraine, Kuban and other areas of wide winter-wheat cultivation. In the early part of the twentieth century the director of the Omsk experiment field Sladkov continued the experiments of the Siberian Cossacks who tried to grow winter wheat. He did not confine himself to ordinary snow-retention measures, but tried to cover the sprouts of winter wheat with a thick layer of straw to keep them warm. But, like many other scientists and practical farmers, he was unsuccessful. In the steppe part of Siberia every other or third year there is little snow in the winter, the first snow falling in December or January, and the temperature dropping to 40° below zero C. In such winters, wheat sown on summer fallow perishes entirely—neither snow retention nor measures to keep the sprouts warm are of any avail.

In recent decades Soviet scientists in Siberia devoted a great

deal of effort to the breeding of "super winter-hardy" or "super frost resistant" strains of winter wheat. But no one has managed to produce strains of winter wheat in which the roots and nodes of tillering could withstand the powerful pressure of the ice crystals when the ice expands in the soil during heavy frosts.

Nor have the tests borne out the fascinating promises of Academician Nikolai Vasilyevich Tsitsin and his disciples, who tried to solve the problem by distant hybridization of wheat with couch grass (Agropyron). The experiments with winter wheat×couch grass hybrids carried on by the Institute for fifteen years, have shown that these hybrids are also unable to withstand the mechanical injuries in winter, and that when sown on fallow they perish just as the ordinary varieties of winter wheat. In many years of breeding work Academician Tsitsin's pupils have failed to combine within one organism chromosomes bearing particles (genes) of hereditary substance with the "super winter-hardiness" of couch grass and with the high yieldiness of wheat. These efforts at combination and treasure hunting amidst a large hybrid progeny, based on the Mendel-Morgan theory, failed to produce a "gem" grain.

As we see, neither agrotechnical methods nor breeding could solve the problem of winter wheat in Siberia when sown on fallow.

The February, 1947, Plenum of the Central Committee of the Communist Party of the Soviet Union pointed out in its decision that the collective farms of Siberia were still destitute of winter-hardy varieties of winter wheat. This does not mean, of course, that there are no such varieties in Siberia at all. Our Institute and the Karaganda State Farm have been cultivating winter wheat for six years now, and in the last few years have obtained high yields. Last year the Omsk Regional Committee of the Communist Party and the Executive Committee of the Regional Soviet mapped out measures to introduce winter wheat on collective farm fields. These measures are based on the positive results obtained by our Institute. This year the Ministry of Agriculture, taking into consideration the favourable results of our experiments, included in the collective farms' plan in the Omsk Region the sowing of winter wheat in stubble on an

area of several thousand hectares. The Ministry of State Farms plan provides for an area of 3,000 ha. in the Omsk Region to be sown with winter wheat in stubble. The collective and state farms of Siberia are thus beginning to introduce the long wished-for winter wheat by using the methods worked out by Soviet agrobiological science. The positive experience of our Institute in the matter of growing winter wheat consists of the following.

In 1942 Academician Lysenko made a momentous scientific discovery—namely, that winter wheat can overwinter in the steppe part of Siberia if sown in the entirely unploughed stubble of spring crops.

Six years of tests of sowing winter wheat in the stubble on the open steppe fields of our Institute near the city of Omsk have shown that not only varieties like Lutescens 329, Alabasskaya, etc., with a high winter-hardiness, but also the less winter-hardy varieties of winter wheat, such as Ukrainka, Novokrymka, Erythrospermum 015, and the like, can overwinter in Siberia. In the past few years we have tested in our experiment fields about fifty of the most widespread and promising varieties of winter wheat. And they all stand the winter satisfactorily and well. Among the tested varieties there are samples from nearly all the regions in our country where winter wheat is grown—for example, Lutescens 329 and new promising varieties bred in Saratov and Kharkov, Erythrospermum 1160 and other Odessa-bred varieties, promising varieties produced by the Mironovka, Verkhnyachka and Nemercha breeding stations, Kuban winter wheats and promising varieties from the Yaroslavl and Alexandrov breeding stations, etc.

It is highly interesting to note that many winter and semi-winter wheats from collections representative of the whole world also winter well when sown in stubble. For example, in the winter 1947-48 more than 300 winter-wheat varieties from all parts of the world, including localities with a severe climate, like Karaganda, and localities in the Mediterranean region, were sown in the course of experiments conducted by the biologist Kostyuchenko. And all wintered well or satisfactorily. Not a single variety perished entirely. They have all given a yield this year. It should be emphasized that the numerous winter-wheat varieties from the world-wide collection overwinter much better

in Siberia when sown in stubble than in any other part of our country when sown on fallow.

Furthermore, it has come to light in the course of the investigations that when sown in stubble late in the fall (from the end of September to October 15) not only winter wheat, but all varieties of soft and hard spring wheats, germinate well and winter excellently in the "shoot" or one-leaf phase. Various varieties of spring barley also overwinter when sown in stubble. In the winter 1947-48 even individual plants of the branched Kakhetia spring wheat (Turgidum) overwintered well. This is a shining example of the creative power of Lysenko's theory of phasic development, which bears the promise of inexhaustible possibilities for directed transformation of the nature of plants by Michurin methods.

In the case of sowing in stubble the soil is compact and structural, and the freezing in the winter does not lead to the formation of numerous cracks, which is what occurs in the case of sowing on fallow. This is why the roots and the tillering nodes of winter wheat are not injured during the winter when sown in stubble. The stubble of the spring crop, in which the winter wheat is sown, excellently protects the young plants against the fierce Siberian winds and holds the snow. When the sowing is done in stubble, new, more favourable conditions are created, which make it possible for winter wheat sown late in August and for spring wheat sown in the beginning of October easily to withstand frosts of 40° C in the air and 17-20° at the tillering nodes in the soil.

Thus an excellent solution has been found to the problem of how to ensure the wintering of winter wheat.

Thanks to Academician Lysenko's discovery, we have scored against the severe Siberian climate.

But the finding of a means to meet the rigours of the Siberian winters is not the whole job. We still have to learn to grow large crops of winter wheat from the seed that has overwintered in the stubble. We know that many scientists and practical agriculturists are still expressing doubt concerning the method of sowing in stubble. They wonder whether the wheat will not suffer from lack of moisture and nourishment, whether it is not likely to be crowded out by weeds. Naturally, there are certain difficulties that must be surmounted before winter wheat can

become a staple crop on collective farms and state farms. And in order to surmount these difficulties the Institute has in recent years conducted special experiments on an area of more than a hundred hectares.

Academician Lysenko recommends, as the best predecessor for winter wheat, the stubble of spring wheat sown on bare, specially fertilized fallow, or on oat stubble free of weeds. Experiments have shown that on such fields the winter wheat overwinters well, but does not always give a good yield. And if the spring wheat is sown in unfertilized fall-ploughed or spring-ploughed soil, the winter wheat sown in its stubble without additional fertilizers will not give a normal yield. In the autumn of 1944, for example, winter wheat was sown in the stubble of spring wheat grown on fall-ploughed virgin soil. The wheat overwintered satisfactorily, but in the spring it developed very slowly. Additional nourishment in the shape of humus, manure and superphosphate applied in the spring proved of little help. The plants pupated, as it were, they stopped growing. The field soon began to become overgrown with weeds. On an area of about 70 hectares the yield of winter wheat on this field averaged in 1945 only about 3 c. per ha. At the end of August 1945 we sowed on the Institute's élite seed farm an area of 112 ha. with winter wheat in the stubble of spring wheat grown in unfertilized fall-ploughed and spring-ploughed land. The wheat sprouted well and practically all of it withstood the winter. But, as in the previous year, it developed poorly in the spring of 1946. It just did not grow. The result was that by July the field was overspread with weeds, and the crop had to be mown for hay. That was a bitter experience. But at the same time, in those same years, on the experiment field the winter wheat sown in the stubble of spring wheat or oats grown on fertilized fallow or on fertilized fall-ploughed land wintered well and gave quite satisfactory yields, and where mineral fertilizers were applied (superphosphate in autumn and nitrates in spring) the yields were good—up to 18-20 c. per ha.

On Academician Lysenko's advice we extended the experiments with the application of fertilizers to winter wheat sown in stubble in the fall of 1946. For one thing, we devoted a great deal of attention to the application of small doses of superphosphate (one centner to a hectare) in the form of granules

placed in the ground in the autumn together with the seed, and to the additional application of 1-2 c. of ammoniacal saltpetre per ha. in the spring. The result was that the 33 ha. of winter wheat sown in the autumn of 1946 gave an average yield of 16 c. per ha. in 1947. The experiments conducted by N. A. Byelozyorova, head of the Institute's agriculture department, have brought to light the high effectiveness of mineral fertilizers when applied to winter wheat. Thus, whereas the plots of winter wheat sown in the stubble of spring wheat grown on unfertilized summer fallow in 1947 gave a yield of 6-8 c. per ha. the same plots, with 1 c. of granulated superphosphate per ha. in the autumn and 1-2 c. of sulphate of ammonia in the spring, gave a yield of 14-16 c. per ha.

The plots where the winter wheat was sown in the stubble of spring wheat grown on bare fallow fertilized with manure and with larger doses of mineral fertilizer (up to 5 c. per ha.) applied to the winter crop, gave a yield of up to 32 c. of winter wheat per ha. Experiments with the fertilizing of winter wheat sown in the autumn of 1947 on an area of 100 ha., both on large fields and small plots, also showed the high effectiveness of the mineral fertilizers. This year, in spite of a severe drought, the wheat will give a mean yield of 12 c. per ha., and on some plots more than 30 c. per ha.

Here are the yield figures for the plots on which the grain this year has already been gathered: On our élite seed farm an area of 11 ha. has been harvested with combines, and we obtained 96 c. of threshed grain, or about 9 c. per ha. On the experiment field the Erythrospermum 1160 variety yielded 28.5 c. per ha., Ukrainka 17 c., and Ferrugineum 1239 about 34 c. per ha.

Here are sheaves of some winter-wheat varieties harvested this year: Ferrugineum 1239 and Lutescens 329. (*Demonstrates.*)

The experiments show that 1 c. of mineral fertilizer applied to winter wheat is compensated by an addition of 3-5 c. to the crop. It has also been established that manure and superphosphate put in summer fallow for spring wheat approximately double the latter's yield and at the same time create good conditions for winter wheat to be sown in the stubble. In such

cases the winter wheat only requires additional nitrogen nourishment in the spring.

The application of mineral and local fertilizers to winter wheat sown in stubble in the autumn of 1947 in Omsk Region collective farms has also demonstrated their high effectiveness. Of 27 collective farms which sowed winter wheat in stubble, approximately a half use ash or superphosphates in the autumn and nitrate fertilizers in the spring. In these collective farms the winter wheat this year has given a good yield, considerably larger than that of spring wheat sown on bare summer fallow. For example, the Kirov Kolkhoz in the Issyk-Kul District, the Bolshevistsky Put Kolkhoz in the Maryanovka District, the Vtoraya Pyatiletka Kolkhoz in the Gorky District and others have this year obtained 20 and more centners per hectare of winter wheat sown in stubble.

On the other hand, in the collective farms where the winter wheat received no superphosphate or ash in the autumn and no nitrate fertilizers in the spring, the growth was poor, the fields became overgrown with weeds, and the yield was very small or the wheat perished altogether.

Examinations of the soil under winter wheat sown in stubble have shown that in the autumn and early spring the microbiological processes are slow, the accumulation of nitrates is small, and there is also less moisture than in fallow and fall-ploughed land. That is why during these periods it is necessary to succour the sprouts of winter wheat by adding mineral and local fertilizers, small doses of which are quite enough to give the plants a good start in the early spring. After that, when in the late spring the microbiological processes in the soil under the stubble develop more intensively, the winter wheat grows well and its development is promoted by the natural fertility of the soil. As for soil moisture, there is more of it in a one-metre layer at the end of May and the beginning of June in the case of winter wheat sown in stubble than in the case of crops sown in fallow. This is apparently due to the condensation of steam from the warm air which intensively infiltrates the soil under stubble via the tiny canals formed by the decomposition of the roots of the preceding spring crop (the stubble).

The drought hardiness of winter wheat sown in stubble has been well revealed this year, when it withstood a severe

drought and hot dry winds, whereas spring wheat, even when grown on good bare fallow, has been considerably damaged.

The most serious problem of winter wheat cultivation when sown in stubble is that of weed control. But it has been revealed that, when nourished in time with additional fertilizer, the winter wheat, which starts growing fast in the early spring, develops faster than the weeds and suppresses them. On the other hand, the weak sprouts of winter wheat which receive no additional nourishment develop more slowly than the weeds and are suffocated by the latter.

That is the picture of interspecific competition in the case of winter wheat sown in stubble.

It does not, of course, follow from this that winter wheat may be sown in stubble on weed-infested plots or that it requires no weeding. On the contrary, it is essential to make sure that the stubble in which the winter wheat is to be sown should be free of weeds, and it must be borne in mind that, just as in all other crops, weeding in the case of winter wheat sown in stubble assures a higher yield.

In the course of the investigation we have found that freshly harvested, undried and poorly warmed seeds of winter wheat produce weak and late sprouts which develop poorly in the spring too. On the other hand, seeds well matured and well warmed in the sun produce early sprouts which develop well in the early spring and grow faster than the weeds.

Experiments conducted in recent years have shown that the higher the spring wheat yield, the higher will be the yield of the winter wheat sown in the stubble of the spring wheat. It is on plots where the spring wheat yielded 25-28 c. per ha. that record yields of winter wheat have been obtained—yields of over 30 c. per ha. The road toward the wide cultivation of winter wheat in Siberia is thus clear. It lies in high yields of Siberia's principal food crop—spring wheat. By properly cultivating the fallows, providing them with enough fertilizer and obtaining high yields of spring wheat, the fields are being properly prepared for the sowing of winter wheat in stubble.

The simplicity and accessibility of this method makes it possible for every collective farm and state farm in Siberia to

grow winter wheat. By solving the problem of winter wheat we go a long way toward increasing the yield of spring wheat, which is Siberia's principal crop. The latter gets an excellent predecessor—bare fallow, which in the European part of the U.S.S.R. is used for winter crops.

After a crop of winter wheat has been grown in stubble the field represents a kind of "cultured annual virgin land." That is why the yield of spring wheat and oats after a winter wheat crop in stubble is 3-4 c. higher than on neighbouring old soil. Thus we find that the sowing of winter wheat in stubble improves the physical properties of the soil and represents a new, additional factor of restoring soil fertility in grassland rotation.

In view of all the favourable points of sowing winter wheat in stubble, the collective farms of the Omsk Region show a great willingness to tackle this important task. While the plan calls for the sowing of several thousand hectares, we now have demands for seed exceeding our possibilities several times over. Our Institute, which works under the direction of Academician Lysenko, is now called upon to help the collective farms in every way to carry out the important Government assignment for the sowing of winter wheat in stubble, so as to enable them to obtain a large crop of winter wheat in 1949—the first in the history of Siberia.

While elaborating methods of cultivating winter wheat by sowing in stubble, the Institute carries on extensive breeding work with a view to producing winter-hardy high-yielding winter wheat varieties with large ears and large grains. A large number of high-yielding varieties have been studied and intervarietal crossings of the best samples have been made. Selections from the most high-yielding samples will be available for propagation in the autumn of this year. I ought to mention that the selection of high-yielding forms of winter wheat against the background of sowings in stubble has proved most effective also in regard to heightening their winter hardiness. Insufficiently frost-resistant but high-yielding wheats from the Ukraine and the Kuban area, when sown in stubble, survived the winter to the extent of from 65 to 75%; but in the third year of our work with them 95-96% survived—in other words, practically all overwintered. These facts demonstrate that by sowing

in stubble in Siberia it is possible to improve the winter hardiness of many high-yielding varieties for the Ukraine, the Kuban area and other sections of the country and, as Academician Lysenko has pointed out, we can successfully produce winter wheats for those sections.

In the process of our breeding work we devote most attention to the alteration (transformation) of the best Siberian varieties of spring wheat into winter wheat. This work is based on the theory of the phasic development of plants—the theory worked out by Academician Lysenko. I shall not dwell at length on the methods of this work, as they are explained in detail in Lysenko's works. I shall only mention that in the first year of our work to transform spring wheat into winter wheat, the sowing in stubble (preferably of oats) is done early in October, and each succeeding generation of these seeds is sown 4-5 days earlier. The idea is that after 4-5 years the spring wheat is to be converted into winter wheat to such an extent as will make it possible to sow it at the end of August or the beginning of September, which is the normal sowing time for winter wheat. To ensure the successful conversion of spring wheat into a winter variety the seeds are well warmed each year before sowing until the complete termination of the dormancy period.

We now have different generations, from first to fourth, of several spring wheat varieties which are being transformed into winter wheat. (*Demonstrates.*) There are among them Milturum 321, Milturum 553, Lutescens 62, Caesium 111, Albidum 3700, Milturum 345, Milturum 290, Caesium 94 and several others. The exhibit sheaves of the first four varieties, harvested towards the end of July, are, as you see, in the phase of complete ripeness. They are well developed and their seeds are of a very high quality.

After the first experiments conducted in Academician Lysenko's laboratory by scientific worker N. A. Byelozyorova in 1943-1944, the Siberian Institute of Grain Husbandry in the autumn of 1945 sowed two hectares to the spring wheat Milturum 321 in the stubble of millet. The sowing was done in the beginning of October. This wheat excellently overwintered in the "shoot" phase, matured simultaneously with winter wheat and in 1946 gave a yield of 16 c. per ha. In the autumn of 1946

five varieties of spring wheat were sown in stubble on an area of 12 ha. In 1947 our Institute's autumn sowing of spring wheats covered an area of 60 hectares. In these past two years the overwintering was not the same on the various plots, ranging from excellent to weak. The yields in 1947 also fluctuated between 6 and 12 c. per ha.; in 1948 the fluctuation is between 3 and 20 c. per ha.

It should be specially noted that seeds of the spring wheat Milturum 321, sown as a winter crop three or four years in succession, overwinter and in the spring develop better than the winter wheat Lutescens 329 sown at the same time. This fact brilliantly bears out Academician Lysenko's suggestion that winter wheat varieties created from spring wheats transformed into winter wheat will be the most winter-hardy and frost-resistant in the world. Here is a sheaf of what was once a spring wheat, Milturum 321, sown in stubble four autumns in succession. This is no longer a spring wheat. It is a winter variety. Its seeds, sown late in May, have practically not eared, as you may see by this exhibit sheaf.

Thus a simple method of producing local Siberian varieties of winter wheat by transforming spring wheats has been discovered and developed. It is a method accessible to all collective farms and state farms.

We are firmly confident that the experiment of changing spring wheat varieties into winter varieties will prove a great scientific and practical success. This method will make it possible in the coming years to produce winter-hardy and high-yielding varieties of winter wheat for the steppe regions of Siberia. We firmly believe in this also for the reason that many collective-farm Michurinists are already experimenting with the object of producing new winter wheat varieties by changing spring varieties. By enlisting large masses of collective farmers to experiment with a view to producing high-yielding Siberian varieties of winter wheat, we are realizing the behests of Michurin, that great coryphaeus of science, who dreamed of the time when the collective farmers themselves, armed with knowledge of Soviet biological science, would breed local varieties for each section of the country.

The discovery that spring wheat winters well in Siberia

when sown in stubble late in the autumn has led to two more important discoveries in modern biology.

First. The crop produced from spring wheat seeds by late autumn sowing in stubble is entirely free of the loose smut *Ustilago tritici*. Our laboratory investigations have shown that the mycelium of the smut which develops in the germinating seeds of spring wheat cannot withstand the low temperatures, whereas the sprouts of spring wheat stand the lowered temperature well. That is why the smut is killed, while the wheat overwinters and gives good yields. Nor has any stinking smut, or fusariose, been discovered in these seeds in the past three years. This example patently shows with what success the natural environment can be utilized to improve cultivated plant varieties.

Second. If seeds of spring wheat obtained from late autumn sowing in stubble are sown next year as a spring crop on fallow or fall-ploughed land, they will yield, as Academician Lysenko predicted, a crop entirely free of loose smut and 4-5 centners per hectare larger than ordinary seeds of the same variety.

Thus a simple method, one accessible to all collective farms and state farms, has been discovered and elaborated, whereby spring wheat seeds can be renovated and improved, and at the same time an effective method of smut control has been found.

Such is the result of the work of a small group of young scientists, Michurinists, who, under the direction of Academician Lysenko, are working, in line with the Academy's plan, to elaborate methods of sowing winter wheat in stubble in Siberia. (*Applause.*)

Academician P. P. Lobanov. I call upon Dr. I. E. Glushchenko.

I. E. Glushchenko (Institute of Genetics, Academy of Sciences of the U.S.S.R.). The Morganist Rapoport has tried to prove, firstly, that Morganism is a materialist science, and, secondly, that the views of our Morganists and of the Morganists abroad are far apart.

It is on these two questions that I want to dwell. Is it really as Rapoport has told us here? Is what he has said the truth?

As the President of the Academy, T. D. Lysenko, has so well shown in his address, the development of biological science has been marked by a struggle between two different trends,

two opposed systems of views concerning the essence of phenomena of life. This divergence of views reflects the contrast between the two philosophical systems—idealism and materialism.

The proponents of idealist views in biology—preformationists, vitalists—assert that the life processes are autonomous and cannot be explained by natural causes alone. The Darwinian doctrine is a powerful weapon in the struggle against the various idealist conceptions. By this doctrine, as Marx put it, "not only is the death-blow dealt ... to 'teleology' in the natural sciences, but its rational meaning is empirically explained." (Letter to F. Lassalle, Jan. 16, 1861.)[1]

Darwin demonstrated that the adaptation observed in the organic world is explained by the action of natural causes—variability, heredity and selection—without any interference of "ultra-mundane" forces. All attempts on the part of idealists to explain adaptation in nature are inevitably in sharp contradiction to the Darwinian doctrine. No wonder therefore that K. A. Timiryazev, as a champion of Darwinism, fought all his life against vitalist doctrines in biology.

That vitalist propositions are incompatible with science is now clear to the overwhelming majority of Soviet scientists. But in a veiled form such propositions are still current among a section of our biologists. And the Mendel-Morgan doctrine on heredity is a case in point.

One of the fundamental questions of biology concerns the nature of the connection between a developing organism and the environmental conditions.

Academician Lysenko has demonstrated with his characteristic penetration that the life processes taking place in a developing organism can only be understood in their interconnection with the conditions of existence. The conditions which an organism requires in the process of its individual development necessarily take part in forming its heredity. Hence it is obvious that changes in the hereditary properties of an organism can only proceed in a way adequately meeting the action of changed conditions.

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[1] Karl Marx/Friedrich Engels, *Ausgewählte Briefe*, Moskau-Leningrad 1934, S. 103.

The substance of the Morganist teaching, however, may be summed up in the statement that the phenomena of heredity are autonomous, that they are independent of the conditions of life.

The Morganists assert that the so-called chromosome theory provides a materialist explanation for the phenomena of heredity, that the chromosomes may be described as the material foundation of heredity. Such assertions may deceive the unsophisticated. As a matter of fact they merely represent an attempt to disguise the real essence of the Morganist conception.

It may be pertinent to point out in this connection that Driesch, a prominent representative of frank vitalism, fully accepts the so-called "materialization" of the phenomena of heredity.

"The material substratum of the phenomena of heredity, as it comes to light in the course of investigations in the sphere of Mendelism," he writes, "we regard as a *means* employed by our autonomous factor. Therefore there is no contradiction whatever between 'Mendelism' and the view that heredity is an autonomous process."

It is necessary to point out in all fairness that representatives of the Morgan trend abroad frankly declare that they have nothing in common with materialism. T. H. Morgan himself wrote in a preface to one of his books:

"Aware also that the use of the term 'mechanistic' [read: materialistic—*I. G.*] may, in the light of the recent developments of mathematical physics, expose my views to the opprobrium of materialism . . . but a careful reading of the text will, I hope, remove to some extent the implications sometimes imputed to those with mechanistic leanings." [1]

The fundamental principle of the Morganists, namely, that the phenomena of heredity are autonomous, directly contradicts the facts known to biological science and practice. Even the Morganists themselves have accumulated many such facts, and the representatives of this trend in biology are therefore compelled to try to salvage the foundation of their theory by changing some of their formulations and devising new hypotheses (lately, for example, the genohormone and plasmogene

[1] T. H. Morgan, Preface to *The Scientific Basis of Evolution*, London.

hypotheses have come into vogue). This explains why some geneticists are inclining towards physiology, why they recognize the biological usefulness of heterozygosity to the organism, etc. This, however, does not alter essentials. The Morganists stubbornly persist in denying that the nature of changes in heredity corresponds to the individual variations arising from the conditions of existence; in other words, they stick to their former *autogenetic* conception.

It is becoming harder for the Morganists with every passing year to defend their idealist positions. Soviet biologists, mastering the science of all sciences, the Marxist dialectical method, and accumulating experimental material, are exposing the idealist essence of the Morgan teaching.

The Neo-Darwinians assert that the problem of heredity is exclusively bound up with the state and action of the chromosomes and the genes contained in them. All the investigations of the Morganists are directed towards the study of the nuclear heredity of the cell. The Morganists disregard all the other components of the cell or relegate some of them to a subordinate role in relation to the nuclear substance. They do not regard variability as an organic consequence of differences in the properties of heredity, as an inherent aspect of heredity. Such an approach to the problem of heredity leads away from the truth; it does not reflect the real development of life. We Soviet geneticists know that nature cannot be regarded "as an accidental agglomeration of things, of phenomena, unconnected with, isolated from, and independent of, each other. . . ." In nature, biological processes take place primarily "as a connected and integral whole, in which things, phenomena are organically connected with, dependent on, and determined by, each other." [1]

A correctly constructed genetics requires the study both of the organism as a whole macrosystem and of the cell as a whole microsystem within the organism. This, naturally, does not preclude but implies the study of individual processes in the development of an organism, of individual cell structures, as part of the general process of investigation.

As has already been pointed out, the proposition that the

[1] И. В. Сталин, „О диалектическом и историческом материализме," *Вопросы Ленинизма*, изд. 11-е, стр. 536.

process of heredity is autonomous is a variety of frank idealism. To escape such a charge our Morganists refer to Muller's work in inducing mutations in fruit flies by the application of X-rays, and, in justification of their own work along the same lines, maintain that with the application of X-rays genetics has overcome its autogenesis. But this is a further corroboration of the fact that Morganism, since it regards every external action upon the organism (action by X-rays or by colchicine) as the conditions of its life, precludes the biology of development, rules out sequence in the development and growing complexity of the hereditary properties of organisms.

The dialectics of nature make it quite clear to biologists that the processes of life include the concepts of the external, that external conditions assimilated by the organism become internal. From this the Michurin science has drawn the conclusion that ontogenetic development leaves its impress on the phylogenesis, that is to say, that characters acquired by an organism in the process of development are transmitted.

However, the majority of geneticists who, on the strength of their fundamental principle, are to be classed as Neo-Darwinians do not agree with this conclusion. What is more, they accuse the investigators who base their work on the mentioned methodological principles of being Neo-Lamarckians.

Corresponding Member of the U.S.S.R. Academy of Sciences Dubinin says that the attempts of biologists "to direct the evolution of organisms by transmission of acquired characters are simply naive ...", because, "... there can be no question of any conformity between the reaction of an organism in development to a given mutative factor and the character of the mutations induced." According to his definition, these naive views are nothing more than mechano-Lamarckism.

A few months ago, at a conference called by the Ministry of Higher Education to discuss a draft syllabus for the course of genetics and breeding, Professor L. N. Delone of the Kharkov Agricultural Institute made similar assertions. When I asked him: "Do you recognize the inheritance of acquired characters?" I and the others present at the meeting received the explicit reply: "No, I don't."

That is the position of the Morganists in regard to one of the cardinal questions of the evolutionary process.

It should be noted that today, as in the past, the Neo-Darwinians are trying to oppose Darwin to Lamarck. Apropos of such attempts, Timiryazev wrote back in 1908:

"I have had occasion more than once (in nearly forty years) to point out that it is unsound thus to oppose Lamarck to Darwin. If Darwin spoke sharply of Lamarck, it was only in regard to his ill-conceived attempt to adduce, as explanations of a form, the psychical, volitional acts of the animal itself; and in this, as the entire subsequent advance of science has shown, he was quite right. As to the dependence of forms on the environment, i. e., the part of Lamarck's doctrine which has retained all its significance, Darwin accepted it from the outset (we need but recall the first draft in his note book of 1837) and as time passed attributed ever greater significance to it. Only the amalgamation of *this aspect* of Lamarckism with Darwinism holds the promise of a full solution of the biological problem." [1]

These statements of Timiryazev are to us Soviet biologists in the nature of a legacy, and we ought to be guided by them.

On this question we, biologists, should be guided by the dialectical teaching on the forms of motion and on the place which the trend in science known as Lamarckism holds in this teaching. As far back as in 1906 Comrade Stalin wrote:

"As for the forms of motion, as regards the fact that, according to dialectics, small, *quantitative*, changes lead in the end to big, *qualitative*, changes, it is a law which holds good for the history of nature as well. Mendeleyev's 'periodic system of elements' clearly shows what great importance attaches in the history of nature to the springing up of qualitative changes from quantitative changes. This is attested to in biology by the Neo-Lamarckian theory to which Neo-Darwinism yields precedence."[2]

Nor will the doctrine of the Neo-Darwinians concerning the mutational process in nature as a phenomenon devoid of causality and direction bear criticism.

Considering that "the mutational process is not directional

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[1] К. А. Тимирязев, Предисловие к книге Ж. Костантена *Растения и среда*, изд. журнала *Русская Мысль*, 1908 г., стр. XI.

[2] И. Сталин, „Анархизм или социализм?“, *Сочинения*, том I, стр. 301.

as regards the laws of the development of the individual, and that it produces a vast number of harmful hereditary alterations which destroy the development of the individual" (Dubinin in 1937), it becomes obvious that evolution has reached a dead end (fortunately, only in the theoretical conceptions of the Morganists).

Even one who is not versed in the fine points of experimental science can see how great is the distance between assertions of this sort and the dialectical method. The dialectical method teaches us that the process of development has a direction, for this process is an onward and upward movement, characterized by transition from an old qualitative state to a new qualitative state, it is a development from the simple to the complex, from the lower to the higher.

Once the Neo-Darwinians dispense with development in nature, there remains for them nothing but to accept the old theory of preformation. And the theoreticians of this trend say so openly.

"The structure of the combinations ... of chromosome molecules in the zygote," wrote Koltsov in 1936, "predetermines the characters of the individual phenotype developing from the zygote—both the morphological (size, colouring, structural characteristics) and the physiological (type of metabolism, rate of growth, fertility, peculiarities of temperament). In this sense we may definitely state that modern genetics fully confirms the old theory of *preformation*." In this statement Koltsov dots the i's and frankly formulates the views shared by all Morganists.

The methodological faults of contemporary Morganism thus show how far removed this trend is from objective truth in the study of the formation of life.

The experimental material of some Morganist geneticists reveals the blind alley reached by the Morgan theory. The researches of the Morganists into the genetics of variegation and phenomena of cytoplasmic heredity shows the fallacy of logically divorcing the part from the whole and of "absolutizing" individual phenomena.

No less forceful a breach in the conception of Morganism has been produced by the latest investigations carried on both in our country and abroad with a view to obtaining directed

mutations in pneumococci, colon bacilli and other micro-organisms.

As far as the Morganists are concerned there is absolutely no explanation for the researches into the genetics of the mouse, for such facts in particular as when young mice susceptible to cancer and nursed by immune females have shown an enhanced resistance to this disease as compared with the normal, and this partial immunity has been transmitted to the offspring.

The experiments in plant hybridization by means of grafting represent the most forceful refutation of the chromosome theory of heredity. Many have spoken of this here, at this session.

Academician Lysenko has shown that the basis for directed variation in the case of vegetative hybridization is the fact that the norm and character of metabolism are disturbed. And we know that, as Engels teaches us, "from the exchange of matter which takes place through nutrition and excretion as the essential function of albumin, and from its peculiar plasticity, proceed also all the other most simple characteristics of life." [1]

The change in the process of assimilation by grafting leads to a change in other processes, including heredity. That is how the old norm of the reaction of an organism becomes disturbed and a new one becomes established.

That this is so has been proved by numerous experiments, both ours and those of other Michurinist investigators, in obtaining vegetative hybrids. On the basis of their study of the laws of development of definite plants the experimenters direct the processes of assimilation, thereby building up a definite, stated type of heredity.

The Neo-Darwinians—both in the past and in the present—starting as they do from groundless assertions that the nucleus of the cell is independent of the vital processes which take place in the whole organism, in other words, that heredity is independent of changes in the type of metabolism—deny outright that grafted hybrids are real.

I need but mention the recent statements of some foreign

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[1] F. Engels, *Herrn Eugen Dührings Umwälzung der Wissenschaft*, Moskau 1946, S. 98.

geneticists like Dunn, Dobzhansky, Goldschmidt, Stern, Sax, and their followers in the U.S.S.R.—Dubinin, Zhebrak, Romashov, Khvostova, etc.

The refusal to recognize the reality of grafted hybrids does not redound to the honour of the present-day Neo-Darwinians, for such hybrids have been produced, they actually exist, and they most cogently reveal the erroneousness of the methodological foundations of Morganism.

In our study of the laws of heredity, vegetative hybridization provides an intermediate link between the phenomena of sexual hybridization and the inherent variability of the heredity of all organisms under the influence of the conditions of life.

The method of vegetative hybridization and the experiments in this kind of hybridization show how fallacious are the attempts to divorce the part from the entire system in which all the biological processes take place, including phenomena of changes in heredity.

Today, after the translation into English of Academician Lysenko's work, *Heredity and Its Variability,* in which the theoretical foundations of vegetative hybridization are set forth, the representatives of Neo-Darwinism abroad are the most insistent in denying the reality of vegetative hybrids.

In the extensive discussion on the subject abroad three tendencies may be clearly discerned. Some authors take advantage of the discussion to make statements pursuing aims that are anything but scientific, aims purely political, aims hostile to the Soviet Union (Sax); others, while taking a positive attitude to many experiments of Academician Lysenko and his co-workers, question Michurin's genetical principles (Dunn); finally, still others deny both the theoretical principles and the experimental data (Dobzhansky).

Particularly characteristic in this respect are the statements of Dobzhansky, an enemy of the Soviet Union, who is regarded as a big authority by our Morganists. The latter often quote him and mention his works in their bibliographies. And not even Academician Schmalhausen is an exception in this respect. In his book, *Factors of Evolution,* he has found no place for Timiryazev, Michurin and the Michurinists, but Dobzhansky is quoted there at length. Now, this Dobzhansky holds that geneticists should not even make experiments to

test the principles of Michurin genetics. "Some people," he writes, "will probably wonder why geneticists do not rush to repeat these experiments. The answer is simple enough. The progress of science would be seriously disorganized if all scientists interrupt their work every time somebody publishes a dubious claim."[1] It is not hard to realize from this statement that the Dobzhanskys have never had anything in common with science. They are afraid of true science. They even dread the idea of repeating Michurin's experiments, because they cannot be easy about their Morganist foundation.

Science knows of many examples when this sort of "politics" led to its degradation and marasmus.

That is the state of Mendel-Morgan genetics we are at present witnessing abroad. Today Mendelism-Morganism is the servant of its class, the militaristic bourgeoisie. In the arsenal of the capitalist world modern Morganism is a weapon, a means for "scientizing" its methods of expansion. Genetic literature abroad teems with articles bearing titles like the following: "Over-Population as a World Problem," "Devil's Cul-de-Sac" (the problem of overpopulation), "The Dice of Destiny," "Politico-Genetics," "Natural Selection and the Birth-Rate," and so on and so forth. The authors of these articles (to the embarrassment of our Mendelists) openly accept Malthus' doctrine, preach Malthusianism as a science. On this basis, they demand a restriction of births in India, Porto Rico and other colonial countries, and at the same time display a suspicious interest in the birth rate in our country and other Slav countries. They do not like the high birth rate in these countries.

R. Cook, editor-in-chief of *The Journal of Heredity,* wrote in 1945: "Whatever world organization finally achieves ratification by the Big Five and their fifty little neighbours, will be only the foundation for the painfully built framework of world understanding and cooperation.... Beyond the fine words and the high hopes ... looms a problem so portentous and so perplexing we prefer to ignore it. That is the question of world population.... In spite of the scared 'pooh-poohs' of

[1] Th. Dobzhansky, "Lysenko's 'Genetics,'" *The Journal of Heredity,* Jan. 1946, p. 5.

the wishful thinkers, it remains as a portentous backdrop of our future."

Cook concludes: "To cut the Gordian knot of this surpassing paradox will involve social inventiveness of a higher order."[1]

What, then, is to be done? This question is answered by an English geneticist, C. Fawcett, in the sense that "if the naturally rapid increase in numbers is not controlled by human acts, the appeal will be to the ancient trinity of 'War, Pestilence and Famine.' "[2]

But different recommendations are made for the ruling class, which is not supposed to be restricted in anything, including birth rate.

An American geneticist, David C. Rife, has written a book, *The Dice of Destiny* (*An introduction to human heredity and racial variation*), which Dobzhansky reviewed in *Science*. Both the author of the book and the reviewer set before genetics the task of helping to develop the genotype of representatives of the ruling classes with a view to adapting them "to different forms of social organization, and to different stations within a given society.... Such stability does not obtain, however, and our Western civilization is particularly noted for rapid transformations. When empires fall, or when ruling classes are overthrown, woe befalls their members unless they change their behaviour speedily. A consistent genetic trend may, therefore, be expected in the evolution of human psychic traits, namely, that genotypes will be selected which permit more and more plasticity, and less and less fixity, in personality characteristics. The end result of this trend will be that the genotypic differences in personality traits will become quite unimportant compared to their phenotypic plasticity. This should not be construed to mean that mankind tends to become genetically uniform; the trend is not toward genotypic uniformity but toward phenotypic plasticity. Natural selection favours above all else the ability to become rapidly adjusted to circumstances which change not only from day to day but, in a modern

[1] R. Cook, "Devil's Cul-de-Sac," *The Journal of Heredity*, July 1945, pp. 208-10.

[2] C. B. Fawcett, "The Numbers and Distribution of Mankind," *The Advancement of Science*, June 1947, p. 142.

society, from minute to minute. Genetic differences may be retained, provided that they permit themselves to be eclipsed by the phenotypic plasticity. . . ."[1]

Arguments in favour of racism, eugenics—that is what rivets the attention of present-day Morgan genetics.

These are the ambitions and aspirations not just of a few individual bourgeois biologists. The entire so-called International Genetics Association is pervaded with such ideas.

In July, this year, the Eighth International Genetics Congress was held. Its proceedings have not yet been published. But the *Journal of Heredity* printed news items about the preparations for the Congress and its character.

Here is the scope of the papers that were to be read at the Congress, as reported in the magazine: "The Organizing Committee has decided that papers which only deal with the pure application of genetics to practical animal or plant breeding must be excluded. The animal breeders have international congresses of their own and these congresses must be looked upon as the proper fora for such papers. . . . In the case of human genetics no limitations will be made with regard to the scope of papers.

"The Program of the Congress is not yet decided. The Organizing Committee has decided to announce only one special section at this time, viz., the Section of Human Genetics."[2]

All this goes to show whom and what Mendel-Morgan genetics serves. The organizers of the Congress and their masters are not interested in problems of animal and plant breeding, in ways and means of increasing the productivity of agricultural crops and animals. Eugenics is the main object of their efforts and the field to which they apply their conclusions.

That is the character of the logical development and present state of "world-wide genetics" so revered by our native Morganists. Yet only two years ago our Morganist, Professor Zhebrak, wrote in the magazine *Science*, addressing the reac-

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[1] Th. Dobzhansky on "The Dice of Destiny" by D. C. Rife, *Science*, 1945, No. 2644, pp. 235-36.

[2] "News of the Eighth International Genetics Congress," *The Journal of Heredity*, April 1947, p. 105.

tionary Sax and his like: "Together with American scientists we who are working in this field in Russia are building up a common, world-wide biology."

Only a year ago Corresponding Member of the U.S.S.R. Academy of Sciences Dubinin, writing in the same magazine, *Science,* explained who those geneticists were and what they were doing.

Extolling the achievements of "world-wide genetics," for one thing the work of our sworn enemies (Dobzhansky, Timofeyev-Ressovsky, and also the Morganists Sturtevant, Gordon, etc.), Dubinin maintains that our home-grown Morganists are not behind the Morganists abroad and in many respects are ahead of them.

While extolling the work of our and foreign Morganists, Dubinin expunged Michurin and the Michurinists from the history of genetics. As far as he is concerned they do not exist.

The Michurinists are proud of the fact that their road is different from that of Dubinin, Zhebrak, Schmalhausen. But the Michurinists ask this question:

How long can you who profess a pseudo science, you who popularize it and serve it, fail to realize that the Soviet scientist and the preachers of idealism in biology abroad travel in entirely opposite directions and their paths can never meet?

Today, after Academician Lysenko's address, you lack courage to defend idealism. But why not muster courage to admit your mistakes, to say loudly and plainly that you have been wrong?

Rapoport's speech indicates that the Morganists, while holding on to their theories, are changing colours. We are witnessing just another case of mimicry. (*Applause.*)

Academician P. P. Lobanov. We adjourn until the evening.

FIFTH SITTING

Evening, August 3, 1948

Academician P. P. Lobanov. We will continue the work of our session. I call on Comrade I. I. Khoroshilov.

Agronomist I. I. Khoroshilov (Rostov Regional Agricultural Administration). The present session of the Lenin Academy of Agricultural Sciences is discussing the situation in the science of biology. It may seem that the controversy between the formal geneticists and agrobiologists is purely theoretical in nature and therefore does not concern us workers in the practical field. As a matter of fact, however, this is not the case.

Our Soviet science cannot fence itself off from production. It must be connected with production by constant living ties and serve the cause of strengthening the economic and political might of our country. Only such a science justifies its existence.

It is a distinguishing feature of our socialist system that all branches of our economy can and must be planned. Hence every useful invention in science is disseminated far and wide.

Soviet agrobiology closely concerns us who are engaged in production; it is in constant contact with us and helps us in our practical work, in obtaining high and stable crops, in the advancement of all branches of agriculture.

The agrotechnical and other methods elaborated by Soviet biologists with Academician Lysenko at their head have received general recognition and wide application in the fields of our country. The vernalization of grain crops and of potatoes, the summer planting of potatoes, the growing of seeds on agrotechnically highly favourable ground for the purpose

of inducing high-yield properties, and many other methods have been tested on millions of hectares, and neither agronomists nor collective farmers entertain any doubt as to their great effectiveness.

The new varieties produced by the geneticists of the Michurin school have yielded excellent results. According to the figures given by the stations of the State Cereal Variety Trial Commission in our region, the yield of Odessa 3 winter wheat, for instance, exceeds that of the former district-standard varieties on the average by 4-7 centners per hectare.

At present it is the task of workers in the field of agriculture to provide such conditions of cultivation for the varieties developed by Soviet agrobiologists as will ensure big and stable harvests irrespective of weather conditions. This requires the rapid mastering of the travopolye system of agriculture, and a close combination in practice of the teachings of Michurin and Williams.

The victory of the kolkhoz system cleared the way for the rapid introduction of science in agricultural production.

The advanced teaching of Academician V. R. Williams on the travopolye system of agriculture, which received every possible support from the Party and the Government, had a powerful effect as soon as it was applied in practice. It took the machine and tractor stations organized to implement the travopolye system in the various zones of our country, including Rostov Region, but a short time to convince even the most doubting of the doubting Thomases of the absolute superiority of the travopolye system of agriculture over the fallow system. These machine and tractor stations showed that only a multiple-course system of crop rotation which includes the sowing of grass and black or early fallow combined with a correct system of soil cultivation, protective forest belts and the use of a system of fertilizers is able not only to maintain but even considerably increase the fertility of the soil and thus ensure large and stable crops.

The collective farms served by the Millerovo Machine and Tractor Station were the first in our region to become convinced of the vast importance of the travopolye system of agriculture. Thus the Novaya Zhizn Collective Farm, which introduced the multiple-course crop rotation system with grass-

growing and bare fallow in 1934, achieved a sharp increase in cereal yields as early as 1941. Whereas in 1934, the initial period of the introduction of crop rotation, cereal crops yielded 4.2 c. per ha. in that collective farm, in 1935 the yield rose to 6.0 c., in 1937 to 8.9 c., in 1939 to 10.5 c. and in 1941 to 11.4 c. per ha. Thus within 5-7 years cereal crop yields more than doubled. But the travopolye system had only just begun to exert its beneficent influence.

Doubtlessly as the fertility of the soil increases, grain and other yields should show further rapid and substantial increases with every rotation of the crops.

It is a characteristic fact that since the introduction of the travopolye system of agriculture in the collective farms under the Millerovo Machine and Tractor Station, the output of grain and other produce per able-bodied collective farmer per work-day unit has risen considerably despite a certain reduction in the acreage sown to grains.

The enormous influence of the travopolye system on cereal crop yields was splendidly confirmed also by other collective farms of our region, and in particular by the Stalin Collective Farm in Salsk District. Here, before crop rotation was introduced, when the land was worked without any system (1921-1933), grain yields averaged only 7.7 c. per ha. After the introduction of fallow crop rotation (1934-1936), cereal crop yields rose to 11.3 c. per ha. and during the last four prewar years, when the travopolye crop rotation system had been mastered, 20.5 c. was the average grain yield per ha. During the four years that travopolye crop rotation has been in operation grain yield has increased 9.2 c. per ha., or 81%.

The derangement of the rotation of crops during the German occupation had an unfavourable effect on field cultivation and the yields of all collective and state farm crops in the Don area. However, wherever the crop rotation system had been sufficiently firmly established before the war and tillage technique had been on an adequately high level, the collective farms rapidly recovered from the devastating effects of the occupation.

After adopting the travopolye crop rotation system the collective farms served by the Millerovo Machine and Tractor Station received in 1944 a 40% higher grain yield than the

remaining collective farms in Krivoi Rog District. During the succeeding years, when the weather was less favourable, the difference in cereal crop yields between collective farms which had mastered the travopolye crop rotation system and those which had not was still greater.

The yield indices of the Stalin Collective Farm, Salsk District, are palpable proof of the exceptional efficacy of the travopolye crop rotation system during the postwar period. Its figures are the more telling since even in comparison with those of the Salsk District, one of the foremost in Rostov Region, the Stalin Collective Farm ranks conspicuously above the former in the level and stability of its crops.

The following are the principal yield indices for the Stalin Collective Farm and Salsk District as a whole, which demonstrate the effectiveness of the travopolye crop rotation system:

Indices	*Stalin Collective Farm*	*Salsk District*
1. Area sown to perennial grasses (in hectares)	722	6,037
Percentage of ploughland	15.0	4.9
2. Forest belt area (in hectares)	194	2,667
Percentage of total ploughland	4.1	2.2
3. Cereal crops in 1943 (in centners)	4.7	3.6
" " " 1944 " "	11.7	10.1
Actual figures 1945 " "	12.3	4.3
" " 1946 " "	14.8	10.2
" " 1947 " "	16.2	7.8
Estimated " 1948 " "	20.0	16.2
4. Gross cereal harvest per hectare of ploughland (1947) (in centners)	8.5	4.3
5. Gross cereal harvest per able-bodied collective farmer (in centners)	57	39
6. Money income per hectare of ploughland (in rubles)	641	183
7. Money income per able-bodied collective farmer (in rubles)	4,470	1,656
8. Milk yield per forage-fed cow (in litres)	1,857	1,297
9. Milk yield recalculated per hectare of ploughland (in litres)	42	26
10. Wool clip per sheep (in kilograms)	4.0	1.8
11. Paid in cash per workday unit (in rubles)	6.50	2.33

As we see, in 1945, a year of unfavourable weather conditions, grain yields in the Stalin Collective Farm, which had proceeded to restore correct crop rotation earlier than others, far exceeded those of the Salsk District as a whole. The indices of this collective farm during the last few years are of still greater interest. In spite of the exceptional drought in 1946, the average cereal crop yield here was in excess of 14 c. per ha.

In 1947, which proved even less favourable for this district than the preceding year, the actual grain yield of the Stalin Collective Farm was 16.2 c. per ha., or 8.4 c. per ha. more than the district average.

It is worthy of note that grain yields on the Stalin Collective Farm during the postwar period stands out for its exceptional stability and persistent upgrade movement notwithstanding unfavourable meteorological conditions during certain years. This is more than can be said of the indices of Salsk District as a whole. Its crops to a large extent react to weather conditions, rising sharply during years of adequate precipitation and falling sharply during dry years. I must add that all the collective farms in Salsk District have introduced travopolye crop rotation, and the crop differences we have observed are to be explained by a more consistent application of the travopolye system of agriculture of the Dokuchayev-Williams complex, in the Stalin Collective Farm. This collective farm has completely restored crop rotation, and it has a closer network of wooded belts, which represent a reliable barrier to withering dry winds.

But the Stalin Collective Farm is distinguished not only for its cereal crop yield. Its mastery of the travopolye system of agriculture has made it possible for it to raise all branches of agriculture to a higher plane, to establish a variety of farm departments and to render it a highly profitable undertaking. In spite of the smaller proportion of land devoted to cereal crops due to an increase in the area under perennial grasses and the complete introduction of crop rotation, the gross cereal harvest per hectare of ploughland in the Stalin Collective Farm exceeds the district figure 100%. It is significant that the production of cereals per able-bodied collective farmer on the Stalin Collective Farm is 60% higher than the district

average and the monetary incomes of its members from the common, collectivized economy are almost 200% higher.

The travopolye system has proved itself a vigorous influence also in cattle breeding. Although grass sowing has been restored only during recent years and forage crops are being reincluded in the rotation system only now, the milk yield of cows on the Stalin Collective Farm exceeds the district average 50%. The great variety of departments on the Stalin Collective Farm, which were made possible by the travopolye crop rotation system, has trebled money payments per workday unit as compared with Salsk District as a whole.

The Stalin Collective Farm figures strikingly demonstrate the endless possibilities enjoyed by the Don collective farmers due to the mastering of the travopolye system of crop rotation.

It is being mastered with fair success by the Ukraina Seed Farm, in Egorlyksky District. Here too grain yields became stable and much higher than the district average. Thus in 1947 the average grain yield on this collective farm was 13.2 c. per ha. as against a district average of 8.0 c. per ha.

Highly instructive results were achieved in 1946 by the Proletari Seed Farm in Zimovniki District. Though there had been a complete lack of precipitation from sowing to harvesting of spring cereals, spring wheat crops grown after perennial grasses yielded more than 10 centners per hectare, which is 2-3 times the yield of these crops on the long-cultivated land of the neighbouring collective farms.

Good soil structure produced by perennial grasses ensured the conservation and efficient utilization of the autumn and winter moisture, which in the given case had been the only moisture available. On long-cultivated land, however, under the same weather conditions, the moisture derived from the autumn and winter precipitations percolated badly in the soil and its storage there was still worse, resulting in miserable crops.

Thus Williams' assertion that on structureless soil the harvest is completely dependent on the frequency of precipitations, i. e., on the elemental forces of nature, was borne out in full; hence it fluctuates so violently from year to year.

It should be noted that the Proletari Collective Farm, the data of which we have been quoting, has been mastering only the first, though a very important, link in the travopolye

system of agriculture—correct crop rotation. Beyond a doubt crop figures of this collective farm would be still higher if it were provided with wooded belts capable of sheltering the fields against the dry winds of the steppe.

Finally I must dwell specially on the highly interesting cereal crop figures of the collective and state farms of Rostov Region for 1947, which reflect the result of the inclusion of grass growing in the crop rotation system. That year weather conditions were not altogether favourable for cereal crops, and in some districts of the region the year was even worse than the drought year 1946.

And yet on the Novaya Zhizn Collective Farm served by the Millerovo Machine and Tractor Station, the spring wheat crop reaped after a growth of perennial grasses on an area of 41 ha. was 30.3 c. per ha.; on the "Fifteen Years of October" Collective Farm served by the same machine and tractor station, the wheat crop harvested on an area of 40.5 ha. sown after ploughing up the soil was 30.1 c.; and on the Politotdelets Collective Farm, Zveryevo District, the team leader Tsukanova reaped a fallow crop of 32 c. of grain per ha. on a spring wheat area of 13 ha.

The Don people had never grown such spring wheat crops in the past. This unprecedented crop of a most valuable foodstuff was due to the perennial grasses, which restored the structure and increased the fertility of the soil. The crumble structure of soil enabled autumn and winter precipitations to be gathered and conserved, and though precipitations were slight during the vegetative period such a large crop was harvested.

In 1947 the big spring wheat crop showed the potentialities of this culture when the travopolye system of crop rotation will provide all the conditions necessary for spring wheat growth. Under the conditions prevailing in the Don area, when the travopolye system is applied in agriculture, spring wheat is capable of yielding a crop no lower than winter wheat.

Finally, the vast importance of the travopolye system of agriculture in combatting wind erosion of the soil must also be noted. In the conditions obtaining in the Don area strong winds cause considerable damage every year. In some years

these winds turn into "black storms," as they are called, which sweep up the tiny particles of soil very rich in nutrient matter and carry them off for great distances, thus baring the seeds and piling them up into drifts.

Black storms were particularly rampant in the spring of this year in the southern districts of the region. During the first half of April a wind of unheard-of violence was raging for seven days, attaining a velocity of 28-30 metres a second. Spring crops suffered greatly or completely perished on tens of thousands of hectares as a result of the carrying off of the top soil. The dust clouds reduced visibility to a few metres and near the forest belts and other windbreaks dust drifts a metre and a half high collected. All the same the travopolye system of agriculture, the entire complex of travopolye crop rotation and protective forest belts proved capable of resisting even such elemental forces of nature.

On the Stalin Collective Farm, which is provided with a close network of protective belts, the dust storm caused no damage whatever. As was established by special observations, storms of even exceptional violence failed to cause wind erosion not only on freshly ploughed land but also on land that had been turned, and even the third year after ploughing. This explains the vast importance of the Dokuchayev-Kostychev-Williams complex, the efficacy of which under the conditions obtaining in Rostov Region can hardly be overestimated.

The data quoted by us undoubtedly do not as yet reflect the full operation of the travopolye system of agriculture, which has been only partly introduced in our region and then only on the foremost farms. Even on the Stalin Collective Farm in Salsk District they plough after an alfalfa crop and not a mixture of leguminous and herbaceous plants. Not only spring wheat but also the crops of the succeeding cultures would doubtlessly be considerably larger if the collective farm used a mixture of leguminous and herbaceous plants which have a more powerful influence on the structure of the soil and the restoration of its fertility.

According to the data of the Rostov Regional Experimental Station, the 1939 spring wheat crop on land unploughed for three years was 11% higher where mixed grasses had been grown than where alfalfa alone had been sown. If the period

during which grasses are used is cut down to two years, as is provided for in the crop rotation system being introduced in our region, this difference in crops owing to the mixing of grasses would be still greater.

The great importance that attaches to protective forest belts as a link in the system of travopolye crop rotation must likewise be stressed. The Rostov Region collective farms proceeded to the planting of such belts comparatively recently; but even these young growths exert an enormous influence on micro-climatic changes in the spaces between the belts and on the crop levels of all farm cultures. Naturally this influence increases with the increase in age of the forest belts and the height of the trees.

The Rostov Region observations have established that even young forest belts, 6-8 years old and 4-5 m. high, facilitate cereal crop increases of 10-15% during ordinary years, and during dry years belt-protected areas furnish crops 1.5-2 times as big as neighbouring unprotected areas.

The data supplied by the above-mentioned Stalin Collective Farm argue eloquently in favour of the efficacy of forest belts when compared with the data supplied by the Gigant State Grain Farm nearby, an advanced agricultural enterprise of our region. This state farm is adequately equipped with the latest machinery, which enables it to carry out all agricultural work at the times most favourable from the point of view of agro-technique and in general to maintain a very high agrotechnical level. It is the usual thing for the Gigant State Farm, after finishing its own spring sowing, to assist its neighbour, the Stalin Collective Farm, in its productive work. And yet each year the Stalin Collective Farm surpasses the Gigant State Farm in cereal crop yields, and generally the worse the weather the greater the discrepancy.

In 1947 the Stalin Collective Farm grain yield was 4 c. per ha. greater than on the Gigant State Grain Farm.

The big and stable harvests gathered on the Stalin Collective Farm are primarily the result of the favourable influence exerted by the wooded belts, which in area and age considerably surpass those of the Gigant farm. In 1947 the Gigant State Grain Farm had only 1.5% of its ploughland covered with forest belts while on the Stalin farm the percentage was

4.1. The ploughland under perennial grasses is proportionately almost twice as extensive on the latter as on the former.

However, the importance of forest belts is not confined to the favourable agrotechnical influence they have on neighbouring crops. On attaining a certain age the belts provide the farms with wood, of which the collective and state farms of our treeless region are greatly in need. By interspersing the wooded belts as far as feasible with fruit trees, fruit yields are obtained in quantities that spell great increases in the money incomes of the collective farms. For example, in 1945 the Lenin Komsomol Collective Farm in Salsk District realized 104,000 rubles from its apricot harvest picked from trees in the wooded belts, not counting in the great quantity of fruit consumed on the collective farm itself.

Such is the tremendous agrotechnical and economic importance of forest belts. At present the attention of the land, Soviet and Communist Party organs in Rostov Region is concentrated on the various questions dealing with forest surveying. We set ourselves the target of completely mastering the travopolye system of crop rotation and of increasing the protective forest belt area to 120,000 ha.

The use of fertilizers is an essential constituent of the travopolye system of agriculture. To Rostov Region this is of particular importance. Not so long ago the theory had received currency even in agronomic circles of our region that the use of fertilizers on the chernozem soils of the Don Region was ineffective. In proof of this one usually cited the comparatively high yields of field culture obtained without the use of fertilizers during years of favourable precipitations. This served as the basis for the argument that agriculturists had only to collect the moisture in the soil and expend it economically. However, as a result of the work of our experimental and research institutions and of our farmer Stakhanovites this false conception of the function of fertilizers was completely disproved. It appeared in practice that the use of fertilizer in the cultivation of many field crops resulted in sharp yield increases.

Observations have established that in improving the supply of nutriment to plants, fertilizers make possible a more economical expenditure of water per crop unit, which under

the conditions prevailing in our region is of particular importance. Experiments have shown that crop increases due to fertilization are of great magnitude, particularly in dry years.

Fertilizers are particularly effective when used as a link in the travopolye system. Perennial grasses improve the water regime of the soil since they rehabilitate its structure, and they therefore greatly increase the effect of fertilizers applied. Every centner of fertilizer used on structural soil is rewarded by a greater harvest increase than if it were applied to ploughed pulverized soil.

The work of the Don Oil Crop Experimental Station has clarified the extremely important problem of the most effective utilization of mineral fertilizers for spring grain crops. As is known, perennial grasses create vast stores of nitrogen in the soil. These stores cannot be completely utilized by the ensuing cultures for lack of phosphorus and potassium. Bearing this in mind, the experimental station followed up the effect on spring wheat crops of phosphate and potassium fertilizers applied before ploughing up the soil. The resulting crop increase for a period extending over four years averaged 2.5 c. of grain per ha. On the other hand, these fertilizers, applied on ploughed up soil, exert their beneficial influence also the following year upon turning it, when the harvest from this field was increased 2.8 c. of grain per ha.

Thus, during the two years of its operation, 60 kg. of P_2O_5 and 45 kg. of K_2O per ha. have raised the yield of spring wheat, this most valuable crop, to the extent of 5.3 c. Such a crop increase is exceedingly great and therefore the use of phosphate and potassium fertilizers for spring wheat should be widely resorted to in travopolye crop rotation.

Rostov Region has its specific features which more often than not prevent it from making complete use of all the favourable properties of its grassland and thus ensure large and stable spring wheat crops. First of all insufficient moisture greatly affects the productivity of the soil. As we know, perennial grasses while rehabilitating the structure of the soil desiccate it thoroughly and deeply. Therefore the abundant nutritive matter left by the grasses in the soil frequently cannot be used up for lack of moisture. That explains why fallow spring

wheat crops are very big when there is plenty of precipitation during autumn and winter, and small when the soil contains but little moisture.

A spring wheat crop planted on the turn will be more stable since the structure remains good for two years after ploughing and is thus able to eliminate the desiccative action of the perennial grasses in the stratum occupied by the spring wheat roots. Raising sod efficacy is the most important problem facing the collective and state farms of the Don area.

As has been established by the latest research of the experimental institutions in our region, early autumn ploughing ensures bigger spring wheat crops in comparison with crops on soil ploughed later for the very reason that early autumn ploughing generally makes possible the storing of rather big supplies of moisture in the ground during the autumn and winter.

But of course early ploughing of the sod is no solution of the problem so important to our region of accumulating moisture. A more reliable and thoroughgoing method of impregnating the soil with moisture after ploughing up the grasses can be achieved only by snow retention. Of this we are firmly convinced.

In most districts of our region winter precipitation totals approximately 80-100 mm., a great part of it being snow. However, this snow is blown off the fields and is therefore of almost no practical importance as soil moisture. And yet snow retention measures skilfully carried out would serve to cover autumn-ploughed land with sufficient snow to yield 200-300 mm. of moisture. This moisture obtained by snow retention is adequate to ensure spring wheat crops of 16-20 c. per ha. even during the driest years. As structural soil possesses the properties necessary for the absorption of all precipitated moisture and its complete conservation, snow retention can be instrumental in considerably improving soil fertility.

We believe we are following the right track to a solution of this problem. At the Rostov Plant-Breeding Experimental Station, in the spring of 1946, they ploughed narrow strips 1-1.5 m. wide at intervals of 25-30 m. on grass plots designed to be ploughed in autumn and sown the ensuing spring. These strips ran from north to south, i.e., at right angles to the prevailing

winter winds. Two rows of high-stemmed plants (maize, sunflower, African millet) were planted on these ploughed strips. When the hay was harvested on the corridors between the tall rows, the big distances between the rows did not hamper the harvesting in the least.

After the second hay harvest the land was ploughed in the autumn for the spring sowing and the rows of plants were left to serve as snow fences. It has been established by observation that these fences are even more effective on autumn-ploughed land than on fallow land inasmuch as a considerable number of the plants, particularly the tallest ones, snap when winter crops are sown at an angle to the fences. But on autumn-ploughed land the tall growths remain untouched and can therefore fulfill their snow-retaining function to the utmost extent. Observations of soil moisture graphs, of fall ploughing with and without stalk fences and comparisons of spring wheat crops enable us properly to appraise this new and original agrotechnical method. Even during the winter of 1946/47, when snow was very scarce, fences considerably increased water stores in autumn-ploughed land and raised the spring wheat crop of 1947 10%. Undoubtedly the efficiency of this agrotechnical method will be still greater under normal conditions.

The decisive importance of moisture for the growing of big and stable crops of field cultures in the Don area and the necessity of using this moisture more rationally under the travopolye system of agriculture compelled us to find another approach for the establishment of optimal standards for the sowing of spring cereal crops.

The research conducted by the Rostov Regional Agricultural Experimental Station and numerous experiments by collective-farm laboratories have shown that the standards for spring grain sowings cannot remain constant from year to year even within a single district or collective farm. They must be set each year, making allowance for the condition of the field, the time limits and methods of sowing and, what is particularly important, the amount of moisture stored in the soil at the time of sowing. A blind application of sowing standards leads to great and unrectifiable mistakes, particularly in sowing fallow crops where water stores without snow retention may prove extremely limited.

It goes without saying that a big spring wheat crop requires a maximum of plants per unit of area, and the provision of each of these plants with the nutritive substances and moisture it needs. Consequently, the greater the quantity of nutritive substances and moisture contained in the ground the greater the number of plants that can be grown per unit of area. But this makes necessary the introduction of higher sowing performance standards. Conversely, when the soil moisture supply is small most of the plants are unable to obtain the requisite amount, which inevitably results in a sharp drop in the harvest.

The water reserve accumulated in the soil by spring plays a decisive part in the cultivation of spring crops. It is these stores of moisture that must be given first consideration in determining sowing standards. In favourable years about 250 mm. of precipitation accumulate in the soil stratum occupied by the roots. These stores can be considerably augmented by snow-retention measures, and thus the development of spring crops can be relieved of all dependence upon the quantity of moisture precipitated during the vegetative period. In April and May the quantities considered decisive for spring wheat crops average 40-80 mm. in the various zones of the region.

Even if we should assume that all the moisture provided by these precipitations is stored in the soil, which practically never happens, and certainly not in the case of pulverized soils, still the store would amount to about 25-30% of the moisture that can be accumulated by spring.

This will make it plain that precipitations during the vegetative period cannot satisfy the water requirements of the plants to any appreciable extent. This must be taken into account when determining spring grain crop sowing standards. The experiments have confirmed the direct dependence of optimal spring wheat sowing standards upon stored up moisture. Since in practical farming it is impossible to work out a complicated millimetrical determination of the moisture stored in the soil it became the custom to estimate it on the basis of the depth of the seepage at the time of sowing. To ascertain this a cross-section of the soil is taken for gauging the depth of the seepage in centimetres. This simple method that every collective farm can follow gives quite a correct idea of the moisture contained in

the ground and can serve as a reliable yardstick for establishing optimal spring wheat sowing standards in every sector.

Depending upon the depth of the soil seepage optimal sowing standards may vary 50% and upward in one and the same district and even collective farm. Therefore one may easily judge of the harvest deficiencies resulting from the use of stereotyped sowing standards. Such approach to this highly important agrotechnical proposition should not be allowed, especially when the travopolye system of agriculture has been mastered. By improving soil structure and introducing snow-retention measures the amount of physiologically useful moisture in the soil can be considerably changed, but it is imperative that sowing standards correspond to the conditions of the soil. Sowing standards determined in this fashion are bound to play an important part in raising the efficacy of the travopolye system of agriculture.

The experience of the foremost collective farms in Rostov Region, which have successfully mastered this system, is convincing proof that the travopolye system furnishes the clue to a further unprecedented development of socialist agriculture. During the past few years proper travopolye crop rotation has been introduced in 75% of the collective farms and in 1948 its introduction will be completed in the main throughout the region. The collective and state farms of the Don area are faced with the urgent task of speedily mastering the introduction of travopolye crop rotation, of planting a maximum of protective forest belts, of advancing to the exclusive use of good-variety seeds, of adopting a correct system of tillage, of extensively using local and mineral fertilizers, and of providing water on the territory concerned for the irrigation of crops and the moderation of climates.

All these measures can be coped with only by our socialist country. The day is not far off when two great Russian rivers, the Don and the Volga, will be united and their waters will flow through the fertile Don chernozem, irrigating hundreds of thousands of hectares sown to valuable food and industrial crops.

The Party, Soviet and agricultural organs of our region are fully aware of the great economic and political importance that attaches to the problem of rapidly advancing the level of socialist agriculture.

For the solution of these problems we agricultural practitioners require the daily assistance of science, and this assistance we increasingly hope to receive from the Lenin Academy of Agricultural Sciences of the U.S.S.R., augmented by a new contingent of our country's foremost scientists. Science and practice must be linked by an indestructible bond. This is one of the most important conditions of the further flourishment of our Socialist Country. (*Applause.*)

Academician P. P. Lobanov. I call on Academician D. A. Dolgushin.

Academician D. A. Dolgushin. It must first of all be noted that the achievements of Soviet selection in the field of creating grain culture strains (and not only such strains) have not the slightest connection with the theoretical propositions of Mendelism-Morganism, as is so often asserted by adherents of this trend in biology. Indeed, if we analyze the fundamental principles by which the vast majority of plant breeders, including those who call themselves Morganists, were guided when they bred new varieties of agricultural plants, we find that they are the same principles and methods that breeders used long before the "laws" of Mendel and even the laws of Darwin were discovered.

These principles amount to selection, the only difference being that some apply selection in the Darwinian sense, active selection permitting of a deliberate accumulation and strengthening of the valuable properties and characters of the plant varieties and animal breeds they are producing, while others use it only as a sieve, "in the Morgan fashion," ignorant of the fact that what they themselves are unable to do nature does for them. This alone explains the chance successes of this sort of selection.

Much of course depends upon the initial material, whether supplied by nature or created by man, and on the skill of the breeder, his knowledge of the peculiar features of the plants, and of the conditions for which the particular variety is being produced. But it could hardly be said that in doing so the breeder was making use of the "laws" of formal genetics. People have been and are producing new plant varieties and animal

breeds both before and after Mendel and independently of Morgan, going by principles that are anything but Morganistic.

But if plant and seed growers make really no use in their work of the theoretical data supplied by formal genetics, can it be said that this "science" did not interfere with their work? Let us see whether it did or not.

The fundamental principle underlying Mendelism-Morganism is the reactionary mystical idea emanating from Weismann that the organism is of a dual nature, that the hereditary "substance" is independent of the body, of the conditions of life. This principle, no matter how it is veiled, no matter what concessions the Morganists may make or with what properties of mutability they may endow the gene—this principle remains the cornerstone of formal genetics.

The conception of the independence of the hereditary properties of an organism as regards the conditions of life has taken deep root in the minds of not only one generation of men. The idea has been propagated from the chairs of higher educational institutions and been inculcated into the minds of mere schoolboys. Even today students at a few universities and colleges still passionately try to defend this truly reactionary view without having any conception of its origins since it was dished up as a strictly scientific truth and was concealed behind a smoke screen of complicated explanations and a "scientific" terminology that sounded passing strange in Russian.

One of the conclusions drawn from this reactionary doctrine by breeders is the theory of pure lines, their permanency and unchangeability, and hence the uselessness of selection among pure-line strains. Besides, the Mendelists boasted of having introduced a new method of selection, the method of individual selection among self-pollinating plants, as if Hallet and the whole Vilmorin galaxy before and after Johannsen and many others had not already utilized this method, and with much greater skill.

But what is true is the fact that the theory of pure lines has entailed the loss in our country of numerous valuable ancient local varietal populations. They were lost because people divided them into pure lines which when tested proved unsuitable in most cases. Thereafter it was announced that the first selec-

tion phase based on selection from local material could no longer yield favourable results since all that could be selected had already been selected and nothing new was being formed, unless of course you took into account those ever more fashionable "mutations" that arose spontaneously by some inner causes and as a rule were of no practical interest, as the Mendelians themselves admitted.

Thus the blame for the loss of many varietal populations and the cessation of work on selection among the so-called pure-line strains must be laid at the door of Mendelism-Morganism.

More. The Weismann thesis, which was proclaimed with even greater "cogency" by Professor Filipchenko and which asserts that the conditions of growth have no effect on the heritable properties of the seeds, produced for some time complete stagnation in seed growing in our country. After all it is no secret to any of our breeders that the élite of grain cultures grown each year at the plant-breeding stations in many instances still differs in nothing from the ordinary seeds of the given strain with which thousands of hectares of collective and state farm fields are sown.

This is another result of the practical application of the Mendel-Morgan teaching.

Every collective farmer firmly believes that élite seeds are seeds that ensure better crops than the common run of seeds.

The Party and the Government have done their utmost to provide for the growing and rapid propagation of élite seeds for the collective and state farms. For this purpose a tremendous network of plant-breeding stations and élite farms has been set up, a State Cereal Variety Trial Commission formed, etc.

But instead of concentrating attention, when growing élite seeds, on improving their breed properties and their fertility, which could and should have been done long ago by making use of the achievements of Michurinist biological science—instead of all this plant and seed breeders confined their attention to the preservation of strain types, to purity of strain. One hundred per cent varietal purity rather than breed and yield qualities became the measure of value of élite seeds.

This can be charged directly to Mendelism-Morganism.

Much can be said of the shortcomings of seed breeding and

each time we find that the principal cause of these shortcomings is the same. It consists in underrating the influence of the conditions in which the breed properties of a variety developed (compulsory annual growing of élites, the absence of tests, etc.).

Another example. Breeders remember the time when experts rogued without the slightest compunction, and with the full support of the law, high-yield rye seed sectors if they happened to be spaced less than one kilometre not only from a field sown to another variety of rye but even sown to the same variety but with ordinary seeds of subsequent reproduction. This was done for fear that the variety might become "biologically" contaminated as a result of possible intervarietal cross-pollination.

Only the convincing data of the experiments of Michurinists conducted under the direction of Academician T. D. Lysenko demonstrated the uselessness of spatially isolating growing rye varieties as well as the biological harm attendant upon this method. One may state with a full sense of responsibility that the Mendelist-Morganists have not the faintest idea of the essence of the biology of fertilization. They are solely responsible for the destruction of seed rye fields.

I shall now return to the question of selection. The demand for pure strains, which is quite clear to everyone, is beginning to be raised also in the selection process where it has unexpectedly been turned into a demand for uniformity in any new strain regarding the morphological characters of its ears. Who and what is behind this demand? Only the experts, it transpires, for they cannot distinguish a new strain from another strain unless, judging by external characters, it is homogeneous. Yet it is as clear as day that varieties are grown not for experts but for kolkhozes and sovkhozes, which must raise big and sure crops. Then again, we have Mendelism in selection raising the imperative demand that a strain produced by hybridization be brought to the so-called homozygous state. This passion for uniformity in the morphological characters of strains is becoming an unwritten law. And today no power on earth can make the State Cereal Variety Trial Commission accept for trial a morphologically variform strain though its yield and other economically valuable characters were double those of other

varieties. Practical breeders know, however, what constant, repeated selection to attain that notorious morphological uniformity with respect to all the minutest characters of the spike leads to. It leads to a debilitation of the strain's vitality, to a diminution of its adaptability to the varying conditions of the environment and, in the end, to the loss of the strain. This largely explains also the feeble success achieved in the growing of new varieties and the improving of the seeds of old varieties.

Again the influence of the fallacious principles of Morganism on practical plant and seed growing must be held responsible for this situation.

Generally accepted views, as they are called, which are forced on breeders by Mendelism and which it is sometimes difficult for them to abandon, quite frequently interfere with their work. I would like to speak about the Morganist concept that hybridization is a simple combination of separate properties and characters of two crossing components in one hybrid organism. It seems very simple indeed: Let me take branched spring wheat as an illustration. If I cross it with ordinary unbranched winter wheat I am bound to find the winter branched form in the progeny. The one supplies the winter property, the other the branched property and everything is in order. The same may be said of crossing couch grass with wheat where it is desired to obtain perenniality from the one and everything else from the other. That is about the generally accepted plan. If you are going to bank on such shuffling (and that is the very thing Mendelism is trying to assert, to demonstrate; it even indicates the percentage of cases in which such a combination may occur, and occur independently of breeding conditions), you will never grow any perennial wheat nor any branched winter wheat either.

A first-generation hybrid is an organism not yet stabilized of a dual nature, and a form with designed properties and characters can be obtained only by growing the organism itself and its progeny under definite conditions. This necessity of establishing specific conditions for the development of requisite properties and characters in hybrids frequently escapes the attention of breeders owing to the circumstance that the ordinary breeding conditions include the conditions necessary for the development of the character or property in question. But not

always so, particularly in remote hybridization. This must be borne in mind. To be successful in his work a breeder must know the conditions required for the development of properties and characters of plants and animals. This task concerns not only breeders; it should be made a major task of our physiologists who unfortunately work very little on this problem.

I have not, of course, covered all of Morganism's sins in retarding the development of our plant and seed growing. Nor have I set myself this task. I only wanted to quote a few examples in illustration of the harm caused us by Mendelism-Morganism, which has held back the development of the creative Michurin trend in biology, with the aid of which we might have discharged our duty to our country in a much shorter space of time.

The speakers have noted both the theoretical and practical achievements of the Soviet, Michurin science of agrobiology under the leadership of Academician T. D. Lysenko. I shall not repeat what they have said. I shall dwell on only one test we are making at an experimental base of the Academy. I am referring to branched wheat. I don't think I am mistaken in saying that we are on the threshold of a new period in Soviet grain growing.

The point is that our present wheat varieties have ceased to satisfy the Stakhanovites of our socialist fields. Our agrotechnique is getting better from year to year; with each year conditions favouring increased soil productivity improve. With this as a background our ordinary winter and spring wheats will soon be unable to make use of the conditions offered them. By virtue of their special features they do not ensure the rapid increase in fertility, they are not adequate to the tasks confronting agriculture in the immediate future and we need new types of wheat with greater crop potentialities, wheats capable of a more productive utilization of the conditions offered them by Stakhanovite agrotechnique.

There is such a type of wheat, namely, branched wheat, whose spike yields up to ten grams of grain (while ordinary wheats under the very best breeding conditions yield no more than two grams). Last year we began working on mastering the production of branched wheat.

Branched wheat is a special wheat. Its yield may be meagre if the usual methods of cultivation are used (as ordinary spring wheats are sown in our country), and you will find no worse wheat anywhere. But if the agrotechnique it requires is supplied it is capable of yielding anywhere from 80-100 centners per hectare. This wheat is an indication of the unlimited possibilities for raising crop yields on our fields, and this is where its exceptional value lies.

As a variety, branched wheat is the result of the surplus nutrition of the former ordinary unbranched wheat. This too can be proved. Hybrids, i.e., organisms possessing a dual nature, can be produced both by sexual and vegetative crossing.

But there is also a third way of producing hybrids, by proper training under conditions which are new for the variety in question and usually not native to it.

Wherein, for instance, do spring wheats converted by late autumn and subsequently winter sowing into winter wheat differ from hybrids in their behaviour? Like sexual hybrids they begin in the second and third generations to produce so-called "segregants" of a winter type, which later fully assimilate this new property by subsequent rearing.

The same thing applies to winter plants when directedly changed to spring plants. Hence it is not so strange as it may seem to obtain branched wheat from unbranched wheat by means of rearing, nor is the hybrid nature of these branched forms, which manifests itself in the multiformity of their offspring, surprising.

Our branched wheat, which we are testing and propagating this year on an area of 12 hectares, is spring grain. It can of course be improved, be better adjusted to the conditions obtaining in our Moscow Region by means of selection and hybridization. At the same time, to meet its requirements, a superior agrotechnique can be established which will serve as a background for training, as conditions necessary for the formation of the variety.

In addition we are faced with the task of growing branched winter wheat. For this purpose it was crossed already last year with soft winter wheat by applying the method of open pollination (by planting castrated plants of branched wheat in a massif of various winter varieties and planting castrated ears

of ordinary wheat in massifs of branched wheat). By this method of crossing we obtained last year about 20,000 hybrid seeds. In order to train hybrids in the direction of greater winter hardiness the sowing was done in the autumn, both in our Gorki and in Odessa. And in order to develop the property of being branched, high-quality nourishment is provided for these plants throughout the period during which they are bred. We already have over a centner of seeds from the interspecific hybrids of the first generation and will be getting that much over again. I believe this is so far the only such case in the history of interspecific hybridization.

Subsequent autumn sowings of these seeds at various points in the Soviet Union and the supplemental nutrition which promotes the development of branchedness will provide us with winter branched wheat.

In order to obtain advance knowledge of the behaviour of hybrids and the nature of their multiformity we have succeeded, ever since January 1947, in raising initial plants, and in crossing and raising first and second generation hybrids. We achieved this by using hothouses in winter and working on a small scale at a rapid pace. As a result we already have tillered plants of the third generation.

The Michurinist science of biology entitles us to pose today the problem of creating high-yield wheats of a new type. It strengthens our conviction that this problem will be successfully solved, not however with the aid of but in spite of Mendelism-Morganism. (*Applause*.)

Academician P. P. Lobanov. I call on Comrade V. A. Shaumyan.

V. A. Shaumyan (Director of the State Kostroma Cattle-Breeding Station). Comrades, the questions under discussion at this session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. are fundamental to Soviet biology and to our agronomic and zootechnical practice. It is this angle from which the labours of the session must be viewed. The attempts of some comrades to reduce the struggle of the Morganist-Mendelists against the Michurin doctrine to unprincipled and meaningless attacks must be discountenanced. This does

not help us in our fight against the Morganist idealists; on the contrary, it throws us off our guard and disorientates us, weakens our struggle against them.

The struggle against the Morgan-Mendel-Weismann theory has been going on for twenty years, but it must be admitted that the followers of this reactionary theory are still rather strong and energetic. During the last two or three years they have become so active that they constantly attack our positions and cause tremendous harm to the science of biology and our creative practical work. The time has come when we Michurinists must shatter this reactionary theory and discomfit its adherents, since it fetters the further development of the teachings of Michurin in the field of creating and breeding new varieties of plants and animals.

I. V. Michurin, this great transformer of nature and outstanding biologist, who worked for many decades on the production of new forms and varieties, proved to the whole world that the formation of the new is the handiwork of man. Michurin was able to raise Darwinism to a new, a higher plane, such as our Soviet system, our socialist economics, requires. This aspect of the case we Soviet specialists and scientists must firmly master and fully understand inasmuch as the theory propounded by Michurin marks a new stage in the development of science.

Michurin wrote: "It must be remembered, I repeat, that plants with respect to all their parts and all the functions discharged by their organisms, under the influence of proper tending perfect themselves in the direction desired by man only gradually, continually, until they reach the stage of full maturity."[1] Michurin demanded active, revolutionary interference in the processes of nature, writing: "We cannot wait for favours from Nature; we must wrest them from her."

T. D. Lysenko elevated the teachings of Michurin, Williams and Timiryazev to a still higher theoretical plane. In the struggle against the Morganist-Mendelists he successfully upheld this doctrine and is constructively developing it further. Thus the Michurin-Lysenko doctrine serves us, the numerous practising agriculturists, as our guiding star.

[1] И. В. Мичурин, *Сочинения*, том I, стр. 159.

I. V. Michurin's theory, which has been fruitfully developed by T. D. Lysenko, demands of us active, purposive control and a directed alteration of the nature of plants and of animal organisms.

Herein lies the strength and power of this theory, which fully answers the requirements of the era of Socialism.

Weismann, Morgan, Mendel, followed by our native crop of Morganist idealists (Schmalhausen, Zhukovsky, Koltsov, Zavadovsky, Dubinin and others), assert the contrary. The advocates of this "theory" maintain that the sex cell is the only carrier of heredity. According to them the sex cell in the body of an organism is isolated; it uses the body as a sort of case which does not cause any changes in it. The sex cell is eternal and invariable and the factors of the external environment exert no influence on the breed- and form-building processes.

Have not the numerous new varieties created by Michurin, Lysenko and their disciples refuted this false doctrine?

One of the followers of the Morganist-Mendelists, Comrade Rapoport, took the floor here trying hard to mislead us, but he will not succeed. He took a step in advance but does not want to go further. I firmly believe that if we intensify our action upon the adherents of formal, reactionary genetics and go about it the right way, they will undoubtedly "mutate," and do so precisely in the direction we want them to. For that reason we must intensify our struggle with them until they realize that it is time for them to stop propagandizing and cultivating these reactionary theories and propositions in our press, colleges, institutes and academies. It is high time they realized that today our Morganist-Mendelists are in effect making common cause with, and objectively—and in the case of some even subjectively—are forming a bloc with, the international reactionary force of the bourgeois apologists not only of the immutability of genes but also of the immutability of the capitalist system.

We must once for all make it clear in our minds that it is precisely we, the people of the Soviet Union, we Soviet scientists and specialists, who are capable, under the socialist system, of solving the problems of biology as of a new science, a science differing both qualitatively and in principle from the bourgeois science of the development of organisms.

This is a task which our opponents do not understand, and they fail to do so only because they do not understand the essence of our socialist order, of our new social system based on a different principle. They do not grasp the fundamentals of the materialist dialectics of Marx, Engels, Lenin and Stalin. Immured in their laboratories, divorced from life, they have lost all political intuition and have forgotten the precepts laid down by the genius of Lenin and Stalin concerning the Party principles in science, the interconnection between theory and practice, the idea that practice in the final analysis determines the correctness or incorrectness of theory.

The "theory" of formal genetics is reactionary to the core as it is bound to lower the role of Soviet man; this theory wants to make us bend our knee to nature; it tries to convert Soviet man into a passive appendage of nature, a placid contemplator of nature who humbly waits for gifts and favours from her.

The principles underlying the Michurin-Lysenko theory are precisely the opposite. This theory raises man to a position of eminence he never attained before, makes him the real master and commander of nature, and indicates Soviet man's place and part in the bold, resolute refashioning of nature.

This in brief is the difference that exists between the followers of the Morganist-Mendelists and the Michurinist-Lysenkoists.

I shall now dwell on the fundamental principles and methods of work in the production and perfection of the Kostroma cattle breed.

You all know that under the direction of Stalin Prize recipient S. I. Shteiman, P. A. Malinina, A. D. Mitropolskaya, N. A. Gorsky and others, the personnel of the Karavayevo Sovkhoz decorated with the Order of Lenin and the members of the foremost pedigree stock farm departments of the kolkhozes in the Kostroma and Nerekhta districts had been working for many years with considerable success on the improvement of local cattle. Jointly they produced a new native cattle breed, the Kostroma cow.

The Kostroma cow is superior to its initial forms with regard to all economically useful indices.

What is our great success due to?

The first and basic condition of success in the formation of

breeds is abundant and skilful feeding of the animals during the periods of their growth, development and production.

A second and no less important factor (I personally put it on a par with feeding) is the skilful and intensive milking of the cow.

The third factor is skilful rearing of the animals, proper tending, inasmuch as all our actions on the organism of an animal are in the final analysis refracted through its peripheral and central nervous system.

Fourthly—on the basis of abundant feeding, intensive, skilled milking and correspondingly proper tending of the animals, we selected the very best pairs and mated them to produce special lines and families, persistently and systematically accumulating and consolidating all valuable and necessary new properties and characteristics in the course of many generations.

For a length of over twenty years the Karavayevo herd received abundant and diversified feed, particularly during the last 10-13 years. Thus, in 1928, the total fodder expenditure per forage-fed cow amounted to 3,256 Russian food units, and in the best years was in excess of 6,000 food units. The expenditure of concentrated fodder per cow was 1,000-2,500 kg. Milkings per forage-fed cow were, correspondingly, 3,389 kg., reaching a maximum in 1940 of 6,310 kg. The live body weight of cows averaged 649 kg. for the entire herd. In this herd more than 70 cows producing upward of 8,000 kg. of milk were raised. Dozens of prime cows yielded 10,000-13,000 kg. of milk. Poslushnitsa II, breaking the world record, gave 16,235 kg. of milk in 387 days with a fat content of 3.92%. Daily milk yields of 45-50 kg. and even 60 kg. are quite usual now. Dozens of cows now exhibit such record live weights as 850 to 950 kg. without special feeding. More than 30 cows of the herd produced during all lactations 75,000-95,000 kg. of milk, while Opytnitsa yielded 100,000 kg. of milk, thus establishing a new world milk yield record.

Test slaughters have shown that all the more important organs of the Karavayevo cows are greatly altered. Lungs, liver, kidneys, spleen, the organs of the alimentary canal (stomach, intestines, etc.) and particularly the heart weighed 1.5-2 times as much as those of ordinary cows. Physiological tests made by

the All-Union Institute of Experimental Veterinary Science under the direction of Comrade A. A. Kudryavtsev, D. Sc., showed the following changes:

1. The Karavayevo cows exhibit higher indices for the cardiovascular system. While low-producing cattle have arterial pressures of 140-160 cm., Karavayevo cows show 180-220 cm. In some of them the pressure is as high as 230 cm.

2. Young cattle, 1.5-2-year-olds, have an arterial pressure of 140-160 cm., i.e., the same as adult low-producing cattle.

3. The venous pressure of Karavayevo cows is 320-450 mm. while low-producing animals usually have a venous pressure of 220-270 mm. Thus a direct connection and dependence has been established between the milk-producing capacity of cows and their venous pressure. The venous pressure of Karavayevo cows is almost twice as great as that of the ordinary run of low-producing cows of other herds.

4. The principal physiological criteria—respiration, pulse and even temperature—are above normal in Karavayevo cows. The pulse is usually supposed to be 55-60; in Karavayevo cows it is 70-86. The bottom low pulse of Karavayevo animals corresponds to the maximum high of ordinary low-producing cattle.

5. The number of respiratory movements of low-producing animals is usually 12-28; in Karavayevo cows it is 28-30 and 40-44.

6. All Karavayevo animals as a rule have a body temperature almost 1° C. higher than others.

7. The basal and total metabolism of Karavayevo cows is as a rule 100% above normal.

8. The amount of air expired by ordinary low-producing cows is usually 40-60 litres a minute; with Karavayevo cows it is 120-140 litres.

9. With Karavayevo cows food passes 1.5 times as fast through the digestive organs as with the ordinary run of low-producing cows (exactly determined by the use of dyes).

10. The sizes of the various organs—liver, heart, abomasum, omasum, blood vessels, etc.—are as a rule 1.5-2 times as big (with reference to the animal's weight) as in low-producing cows.

11. The heart is more conspicuous than all the other organs because of its great size and powerful muscles.

12. The udder is of astounding weight, 15-18 kg., while the udder of a low-producing cow usually averages 0.5-1.5 kg.

What do these data indicate? They fully corroborate the propositions advanced by us concerning the great changes that have taken place in certain organs of the cows and in their organisms as a whole. The physiological functions of separate organs and of these organs in their entirety will give rise to the type of highly productive animal which the Karavayevo Sovkhoz has bred.

These experiments are only the first steps in the study of the physiological peculiarities of our herd. A close study of the liver, heart, mammary glands, udder, blood vessels, lymphatic ducts, etc., is still to be made but it cannot be disputed that such a study will reveal many highly interesting details concerning the changes in these organs, changes which have been directly effected and conditioned by the system of abundant feeding and intensive milking.

Particularly striking are the changes that go on in the lifetime of the cow from the first calving until it reaches productive maturity. More striking still are the changes that occur in the course of several generations. The udders of many record-breaking cows have circumferences of 1.5-1.85 m. Udder weight (after slaughter) have been as high as 22-25 kg.

The pliability and capacity of the udder to undergo sharp changes during comparatively short periods of time, that is to say, the lifetime of a cow, is in my opinion most telling and indisputable proof of the unlimited possibilities of environmental purposive action on the organism of dairy cattle. I must therefore emphasize in particular the great importance of skilful and intensive milking as a necessary condition of use, a point which Darwin frequently insisted upon in his day.

It must be stated most emphatically that just as it is impossible to produce a good race horse without training and to successfully organize the fattening of cattle, and particularly of hogs, without limiting their movements, so record milk yields and perfected dairy herds are utterly inconceivable without skilful and intensive milking and without skilful tending of the udder.

For twenty years we have been carrying on observations on a mass scale for a study of the process of milking on the Me-

shcheryakovsky Milk and Meat Sovkhoz, and on the Kommunarka and Karavayevo Sovkhozes as well as on kolkhoz livestock farms. It has been established that in order to obtain one litre of milk the milkmaid must produce over a 100 squeezes with her hand. A cow yielding 6,000 kg. of milk would therefore be subject to more than 6-7 million such irritations during her lifetime. To this must be added the washing of the udder with warm water 3-4 times a day and rubbing it dry with a towel, and the massage of the udder during milking. It will then become plain that this important organ of milk secretion is persistently and perseveringly being acted upon daily with constantly increasing intensity for the period of 14-15 years and more. I consider the factor of feeding and milking a single process. Feeding and milking are correlated and condition each other. Abundant feeding ensures abundant milk formation and the latter can be obtained only when the cow's organism is forced by means of intensive milking to change from the formation and storing up of fat and meat, which leads to fattening, to working up the bulk of the fodder she receives into milk.

The cow's udder, which is one of the most important parts of her organism, changes gradually under the influence of our action, which in turn causes corresponding changes in the entire milk forming apparatus. Step by step the cow's organism is changed and adjusted to the requirements made by man upon the cow's udder with indefatigable and ever increasing insistence. The efficacy of the laws of exercise, of correlation between growth and development and of the correlative interdependence between the cow's udder (through the process of milking) and the animal's organism as a whole is expressed perhaps with greater force, emphasis and plainness in the udder than in any other organ or body part of the animal. This aspect of the case must be stressed for the additional reason that the exercise factor in plant organisms affords no such palpable and undisputable examples.

We state on the basis of many years of observation that all these changes in the udder of a milch cow are a direct result of our external actions. Photographs of the udders of Amazonka and Barkhotka show very distinctly the character and strength of the influences we exert.

In his immortal work, *The Origin of Species,* Darwin wrote: "In animals it [habit] has a more marked effect; . . . The great and inherited development of the udders in cows and goats in countries where they are habitually milked, in comparison with the state of these organs in other countries, is probably another instance of the effects of use."[1]

The work-force of the Karavayevo Sovkhoz and the members of the foremost kolkhoz pedigree stock farms began their work of cattle perfection with the following maximum productivity indices—2,500-4,000 kg. of milk—in the original breeding group. Then they achieved indices as high as 4,800-6,300 kg. of milk in the prime cattle herds. The best herd ancestresses had maximum yields of 4,500-5,400 kg. of milk, while today many dozens of cows show milk yields of 10,000-14,000 kg., at times topping the 16,000 mark.

All this work of many years goes to confirm that there is no law of the stability of hereditary properties and factors.

I must elucidate here one more point. When after adducing a whole string of facts you finally have the formal geneticists cornered they put up something like the following "argument": All right then, we'll admit that changes actually did take place, even astounding changes; but you see, these changes are not the result of the action of external conditions; they had existed, in latent form, in the hereditary factors, in the original gene fund; they have really only been "disclosed" by you. Only the gene fund explains these changes and therefore you have really not created anything new here. The most typical of those who reason thus is Professor Kislovsky of the Timiryazev Academy. For five or six years he has been and still is trying to prove to us that no new variety has been produced. We had Swiss-Brown cattle and they have remained Swiss-Brown cattle, and if you improve them all you can call them is improved Swiss-Brown cattle.

In his wrath Comrade Kislovsky went as far as proposing to call the breeders of this race to account. Here we have one of the odious effects the reactionary theory of the immutability and eternity of hereditary properties has led to. Besides, this is a palpable illustration of how the Morganist-Mendelists

[1] Charles Darwin, *The Origin of Species*, London 1861, p. 11.

"help" us practical workers in our difficult and complicated work.

The breed and the results of the many years of labour put in by the work-force of the sovkhoz and the foremost collective farmers were recognized only after the energetic intervention of A. I. Kozlov, P. P. Lobanov, S. F. Demidov, E. M. Chekmenev and, finally, Andrei Andreyevich Andreyev. Of the scientific cattle breeders E. F. Liskun alone supported us.

But for the intervention of the above comrades this matter, of such importance to the whole country, would have been frustrated and thus deprived of the tremendous support and attention which are one of the most important conditions for carrying on the creative work of the millions of advanced agriculturists.

Let us return to the pith of the question. If one assumes the position of the Morganist-Mendelists one must obviously presuppose that once upon a time the ancestors of our cows had really great hereditary factors: 15,000-16,000 kg. of milk, 800-900 kg. of live weight, cow udders weighing 20-25 kg., and so forth. A detailed study of the genealogy of our herd for a period of forty years failed to confirm anything of the kind, and could not do so. Perhaps we must go further back in time. One must assume then that many thousand years ago dairy cattle somehow or other came into being endowed with definite hereditary factors which we have but now "disclosed." The question then arises who "deposited" these genes and hereditary factors in the souls or bodies of our cows, and when? What need was there for such great milk yields and weights, what historical, natural or biological adaptational necessity dictated or occasioned daily cow milk yields of, say, 50-60 kg.? Beyond all dispute the existence and development of the offspring (calf) required no more than 200-250 litres of milk, not over 3-5 litres a day. How, then, explain the presence of genes for the production of 15,000-18,000 kg. of milk a year, or 50-60 kg. a day? Could any such animal exist at all at any time for long? Absolutely not. Its weight, form, bulk and heavy udder would have deprived it of the possibility of even relatively slow locomotion, not to speak of fast running. This circumstance alone would have made it splendid and easy game for even the most feeble and slothful of the beasts of prey.

The absurdity of such a supposition or assertion is obvious. It is quite clear that the present-day milch cow is the result of a historically protracted process, the result of human labour. And everything connected with raising the productivity of animals is the result of systematic and persistent action by man continued for many ages upon the organism of the cow.

During our many years of work which yielded us considerable success we have steadily been guided by the teachings of that great transformer of nature, I. V. Michurin, and his foremost continuator, T. D. Lysenko. We were guided by the directives of the great scientists M. F. Ivanov and P. N. Kuleshov, innovators in the field of animal husbandry. "Fodder and feeding," M. F. Ivanov used to say, "exert much greater influence on the organism of an animal than breed and origin."

On the question of perfecting the meat and milk production capacity of animals P. N. Kuleshov wrote: "... it may be said with certainty that in the development of these two kinds of useful productivity external influences played a much more considerable part than artificial selection. The formation of milk and dairy cattle breeds is possible only when the animals are provided with the requisite conditions, the most important of which are fodder, climate and exercise of the organs."[1]

P. N. Kuleshov then goes on to say that by means of selection and mating of the corresponding pairs it is possible to fix the useful characters and create a new breed, "... but without the requisite feeding and exercising of the organs this objective is absolutely unattainable."[2]

Thus we see that the external factors and internal hereditary properties of the organisms are directly interconnected and constitute a unity of opposites. Present-day hereditary qualities of a given species or individual are the result of gradual quantitative changes over a great number of years brought on by the influence of the external environment. The variations which

[1] П. Н. Кулешов, *Теоретические работы по племенному животноводству*, 1947 г., стр. 56.
[2] *Ibid.*

bear the character of physiological changes in the functions of particular cells, cell structures, separate organs or the whole system, or chain, of organs become inherent, organically necessary for the organism or species concerned and therefore change the hereditary nature of the animal. As these changes accumulate and become settled, they gradually, and at times quite imperceptibly to the bare eye, enrich from generation to generation the hereditary nature, content and essence of the present species, breed or variety.

The hereditary properties in the new, succeeding generations are manifested in their turn to the extent to which they managed to become fixed in the past due to the influence of the conditions under which they arose and developed and the conditions which they encounter in the succeeding generations. Therefore the work of perfecting breeds and strains requires an organization which can assure uninterrupted and steadily increasing action of the corresponding environment in close combination with unintermittent bold and creative selection and mating of the corresponding pairs.

A few words on Lamarck. We are often charged with Lamarckism and Neo-Lamarckism. The Morganist-Mendelists who accuse us of Lamarckism fail to note that Lamarck is irrefutably against them when he speaks, and quite rightly so, of the influence of external conditions on the development of plant organisms. But where Lamarck tries to deviate from this absolutely correct proposition to explain the evolution and formation of animal organisms he assumes positions that are out-and-out idealistic. All the theories and propositions laid down by Weismann, Morgan, Mendel and others concerning the sex cell, its specific nature and invariability, mutations, autogenetic phenomena, etc., are in substance nothing but absurd unscientific propositions advanced by Lamarck which he tried to apply to animal organisms. The "inner desire," by which Lamarck tries to explain form-building and development in the animal world serves in essence as the basis of the mutation theory, autogenesis, autonomism and specific nature of the sex cell in its desire to remain eternally unchanged.

The only difference in this question between Lamarck and the Morganist-Mendelists is this, that according to Lamarck

these "desires," in animals, led to variability while with the Morganists this "desire" resides essentially in the sex cell in the case of both plants and animals, which makes them invariable, eternal. We therefore look upon Lamarck as a two-edged weapon with which we can strike at the formal geneticists quite effectively, using the proper edge as the occasion may require.

The formal geneticists have done us tremendous harm; they are trying to disarm millions of the foremost agriculturists who with utmost devotion work indefatigably and creatively day and night to increase the wealth of our country.

We must now finally and irrevocably take this unscientific and reactionary theory down from its pedestal. Unless we intensify our "external action" upon the minds of our opponents and create for them the "proper environmental conditions," we shall of course be unable to remake them. I am fully convinced that if we guide ourselves by the only correct theory, the theory of Marx, Engels, Lenin and Stalin, and take advantage of the tremendous care and attention which the genius of Stalin bestows upon men of science, we shall undoubtedly be able to cope with this task. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician M. B. Mitin.

Academician M. B. Mitin. Comrades, this general assembly of the Lenin Academy of Agricultural Sciences is summing up the struggle between two trends that has been raging in the field of biology in our country for many years. This struggle is of vital significance, and it centres around fundamental problems of biology. The issue is: shall biology be further developed and the practical workers in agriculture and livestock farming be equipped with powerful, scientific, effective methods of raising our socialist agriculture to a still higher level, or shall we engage in vain, unscientific scholastic "researches" which are not only of no benefit to our country and our state, but mislead our practical workers in agriculture? The issue is: shall we further develop our Soviet, consistently materialist, Michurin trend in science, which has enriched biological theory with discoveries of immense scale

and importance and marks a qualitatively new step forward in the theory of evolution, or shall we slavishly accept the unscientific, idealistic concepts of foreign bourgeois "authorities" who are hacking at the roots of the theory of evolution?

Tracing the scientific work, the literature, the practical results attained, the discussion and statements of the contending representatives of biology, it is possible clearly to perceive that two diametrically opposite trends have arisen in our country. One of them is rightly called the Michurin trend, after its initiator, the great naturalist and transformer of nature, I. V. Michurin; the other is the reactionary, idealistic Mendel-Morgan trend, founded by the bourgeois scientists Weismann, Mendel and Morgan.

The methodology of the Michurin trend is based on the principles of dialectical materialism; it constructively develops Darwin's theory of evolution, while casting aside his one-sided, fallacious and obsolete propositions. This trend is closely connected with life, with practical socialist agriculture; it is working successfully to improve the old and to produce new varieties of plants and breeds of animals; it is promoting the science of biology, and is, in the true sense of the word, a popular trend, for it maintains daily, direct contact with our kolkhozes and experimental stations, our plant and animal breeders, our agronomists and front-rank workers in the kolkhozes.

The Mendelist-Morganist trend in biology, on the contrary, is continuing and developing the thoroughly idealistic and metaphysical theory of Weismann, namely, that there is a difference in principle between an alleged immortal and continuous "hereditary substance" and the so-called mortal "soma." No matter what reservations concerning Weismann's theory the representatives of the Mendel-Morgan trend in this country may attach to their statements, in substance, their theoretical foundation, their theoretical point of departure, is Weismannism, that reactionary and utterly bankrupt theory which denies that man can actively influence directed alteration of plant and animal organisms.

For many years the representatives of the Mendel-Morgan trend have been engaged in fruitless laboratory experiments divorced from life, divorced from the requirements of the

people and of socialist construction. It is an anti-popular trend in science.

To what disgusting monstrosities this trend leads, was shown in his address by T. D. Lysenko, when he quoted as an example the researches of Dubinin on the influence of our Great Patriotic War upon the chromosome apparatus of fruit flies.

The name Dubinin deserves to become the synonym for divorcement of science from life, for unscientific researches in theory, for pseudoscientific Mendelist-Morganist formal genetics, which encourages this kind of "research."

As a definite bourgeous trend in biology, Mendelism-Morganism arose in Western Europe and in America at the end of the last and beginning of the present century.

The representatives of the Mendelist-Morganist trend—Morgan, Johannsen, de Vries, and others—directed all the conclusions they drew from their researches against Darwin, against his theory of evolution, against his theory of natural selection. The subsequent spread of Mendelism-Morganism clearly goes to confirm the fact that the spearhead of this trend in biology is turned against the theory of evolution, against the very idea of the evolution of nature.

Darwin based his theory on the idea that living nature undergoes continuous change; but the Mendelist-Morganists conduct their researches for the purpose of discovering "arguments" to prove that the inherited characters of living organisms are immutable. The assertion that the inherited characters of organisms are immutable, however, logically leads to the conception that living nature as a whole is immutable.

This anti-Darwinist, anti-evolutionary theory has also found expression in the field of biology in our country. As is known, one of the most active propagandists of Mendelism-Morganism in this country at the end of the 'twenties was Professor Y. A. Filipchenko. He wrote:

"... the theory of variation, and the whole of present-day genetics of which it forms a part, is not inseverably connected with the theory of evolution ... the geneticist can calmly work in his own field without even mentioning evolution.... Fully conceivable is also the standpoint of ... the geneticist,

who is profoundly agnostic as regards the problems of evolution."[1]

"... the theory of evolution," he wrote, "has always been and will be just a hypothesis, because the transformation of species is not a phenomenon that one can see."[2]

In the opinion of this same Filipchenko, everything speaks "in favour of autogenesis—of evolution under the influence of certain forces inherent in the organisms themselves."[3]

Such were the views of one of the active representatives of the Mendelist-Morganist trend in this country.

Another ardent champion of Weismannism and autogenesis was Professor N. K. Koltsov (a eugenist and advocate of race theories in biology). Proceeding from the theory of autogenesis and "pure hereditary lines," Professor Koltsov presented in his writings in the guise of science the most reactionary and preposterous piffle. He wrote, for example: "Those who made the history of Europe belong to a few hereditary lines, and these lines are closely interrelated with each other by consanguinity."[4]

The theories of Weismann, Mendel and Morgan also found expression in the works of Serebrovsky, Dubinin and Zhebrak, and are now being elaborated in the works of Schmalhausen. The "works" of Academician Schmalhausen are today the chief works that represent and express Mendelism-Morganism in this country.

Sometimes, the representatives of formal genetics express resentment when they are called Mendelist-Morganists. They say that they do not entirely follow Mendel and Morgan, and claim that in such and such articles and at such and such meetings they made such and such reservations. But what are we to do when Academician Schmalhausen himself, in his latest work *Factors of Evolution* (1946), writes: "Mutation is always something newly acquired by the organism, whereas modification is a slight superstructure—a variant of already existing

[1] Ю. А. Филипченко, *Изменчивость и методы ее изучения*, 1929 г., стр. 249-50.

[2] *Ibid.*, p. 250.

[3] Ю. А. Филипченко, *Эволюционная идея в биологии*, 1926 г., стр. 202.

[4] Н. К. Кольцов, „Генеалогия Ч. Дарвина и И. Ф. Гальтона", *Русский евгенический журнал*, том I, вып. I, 1922 г., стр. 69.

organizations. Mutation is transmitted to offspring in accordance with strict laws. These laws were discovered by G. Mendel. They have been, in the main, confirmed, and have been subjected to very profound analysis by present-day genetics (particularly in the school of T. H. Morgan)."

We thus see that Academician Schmalhausen refers to Mendel and Morgan as the chief authorities who discovered the fundamental laws of mutational variations.

Despite the numerous reservations that are to be found in Schmalhausen's books, his conceptions, his main point of view, as expounded in his works, particularly in his book *Factors of Evolution,* are a reproduction of Weismann's autogenetic conception in biology. The basic concept he lays down to explain the principles of evolution is that of the "substratum of phylogenesis."

The task of phylogen is, as is known, to reveal the laws of nature which determine the origin and development of different species of organisms. Naturally, therefore, the "substratum of phylogenesis" is the principal vehicle of heredity.

I will quote several statements by the author of *Factors of Evolution*. Academician Schmalhausen writes:

"... *nuclear structures* are the specific *substratum of phylogenesis* in which are fixed all hereditary variations, i. e., all variations in the norm of reaction, including ontogenic variations, variations in the organization and its characters, and variations in the adaptive reactions (modifications) of the individual organism."[1]

Thus, the "substratum of phylogenesis" contains everything: hereditary variations, modifications, ontogenetic variations and mutations.

"Substratum of phylogenesis" is a metaphysical and scholastic concept. In essence, it is merely another term for Weismann's "hereditary substance," a repetition of the old, reactionary ideas of the "genofund," which the representatives of formal genetics fussed around with, and which were exposed in this country long ago.

In complete harmony with Weismannism, Schmalhausen

[1] И. И. Шмальгаузен, *Факторы эволюции*, стр. 74.

denies the essential importance of the external factor in the evolution of organic forms.

"The external factor," writes Academician Schmalhausen, "on reaching the threshold of reactivity of the organism's tissues, merely gives the first impetus which sets in motion the internal mechanism of a definite complex of form-building processes. It determines neither the quality nor the scale of the reaction. At best (and then not always) the external factor merely determines the time and sometimes the place of its occurrence."[1]

Such, in the opinion of Academician Schmalhausen, is the role of the external factor in evolution. It is not the cause, it determines neither the quality nor the scale of the organism's reaction to environment. Everything is contained in the so-called "substratum of phylogenesis."

Further on, Academician Schmalhausen writes: "With autonomous development, the role of the external factors is even more reduced than with self-regulated form production. The internal factors of development acquire fundamental importance. The external factors lose their function as the starting mechanism—all the morphogenetical reactions come under the influence of the internal factors."[2]

The author admits that the external factor may play at least some role as the "starting mechanism" and then denies even that role to it. With the "autonomous development" of organisms, all the "morphogenetic reactions" take place only "under the influence of the internal factors."

Such is Academician Schmalhausen's standpoint as expounded in his book *Factors of Evolution*. It very clearly shows that despite certain reservations, or even critical remarks, about Weismann and Mendel, Academician Schmalhausen's fundamental propositions, the main theoretical basis from which he proceeds, his position is obviously that of Weismannism.

In his *Factors of Evolution* and other of his works, Academician Schmalhausen elaborated pernicious, unscientific theories concerning the correlation between "wild" and

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[1] *Ibid.*, p. 82.
[2] *Ibid.*, p. 84.

"cultivated" forms in the organic world. He is of the opinion that the wild forms contain undiscovered, unrevealed, or, as he puts it, "reserves of mutations," "reserves of mutational variations." The formation of varieties and breeds is nothing more than the disclosure of these reserves of mutations deposited in wild forms *a priori*. It follows, therefore, that cultivated species in the plant and animal world are, properly speaking, not the products of cultivation, not the product of the vast creative labour of generations of men, of theorists and practical workers in agriculture and animal husbandry, but merely the "disclosure" of the reserves of variability deposited beforehand (it is permissible to ask here—by whom?) in the "wild" forms.

Comrade Shaumyan, who spoke before me, told us about the splendid results our stockbreeders have obtained in rearing highly productive breeds, about increased milk yields from cows unprecedented in the livestock industry, and thereby refuted the pseudoscientific and pernicious inventions in which the representatives of Mendelist-Morganist biological "science" indulge.

Describing the general process of evolution, Academician Schmalhausen says that a general reduction of the "reserves of hereditary variations in the population" is gradually taking place. "This process," he writes, "of loss of evolutionary plasticity of forms I call 'immobilization.' "[1]

As a consequence of this "immobilization," the variability of later forms becomes considerably lower than that of the wild forms of plant and animal organisms. Further on he writes: "Immobilization also takes place with the standardization of breeds and varieties produced by man."

Thus, according to Schmalhausen, the breeds and varieties produced by man, breeds and varieties useful and necessary to man and, therefore, standardized, are undergoing a process of "loss of evolutionary plasticity" and "immobilization," in other words, plant varieties and animal breeds are deteriorating, are losing their "wild" vigour and "charm." What should we

[1] Академия Наук СССР, *Юбилейный сборник*, посвященный 30-летию Великой Октябрьской социалистической революции, том II, стр. 256.

call such "theories" and such a "science"? In essence, it is a theory of limits, which hinders the further development of socialist agriculture and, if it is not resisted, is capable of demoralizing and disorganizing our agricultural cadres.

As is known, Academician Schmalhausen is the author of what is called "stabilizing selection." What does he mean by "stabilization"? He writes: "The essence of stabilization does not lie in the transition from the non-hereditary to the hereditary principle, or in the substitution of the latter for the former, nor does it lie in transition from phenotypical to genotypical variations."[1] We see, therefore, that he adheres to the typical Weismann point of view of a rupture between phenotype and genotype.

Academician Schmalhausen's *Factors of Evolution* also contains references to dialectical materialism, but these are mere words; as a matter of fact, the entire methodology upon which this book rests has nothing in common with dialectical materialism. That book is metaphysical and idealistic. The methodological basis upon which the author's entire conception rests is the notorious equilibrium theory.

I will quote several of the statements of Academician Schmalhausen. He writes: "If we are to speak of the *nucleus and its chromosomes* as a system ('the balance of chromosomes' and gene equilibrium), then it must be admitted that it is in a state of *equilibrium that is not very mobile, but at the same time relatively not very stable.*" "The stability of the cell," he writes further, "is determined by the continuous *interaction of the nucleus* (as a system which is in a state that is relatively not very mobile, but also not very stable) and the *plasm* (as the regulating system which is in a very mobile and stable state)."[2] We see, therefore, that the author employs all the principal categories (stable and unstable equilibrium, etc.), of the Bogdanov-Bukharin theory of equilibrium.

I could quote many more postulates from Academician Schmalhausen's *Factors of Evolution,* but I think that what has already been quoted at this session is convincing proof that the author's conceptions are fallacious and unscientific.

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[1] *Ibid.*, p. 265.
[2] *Ibid.*, p. 75.

And after all this, people are to be found who write literally the following:

"The literature on biology of the last decades dealing with the problems of evolution, contains nothing like the profound analysis of facts and their profound and fruitful generalization as that carried out by I. I. Schmalhausen." This was written by Professor Polyakov in the magazine *Sovietskaya Kniga* (*Soviet Book*) for June 1947. This shows that we have in this country not only scientific workers who write things of this kind, but also magazines which publish them.

In opposition to the formal-geneticist, reactionary, idealistic trend in biology, the Michurin trend, led by Academician T. D. Lysenko, has grown up, become strong and has widely developed in our country.

I want to remind you that, in speaking of the prospects of development of Darwinism, K. A. Timiryazev said that the further and higher stage of this development would be the discovery of the laws and methods by which it will be possible to "mould organic forms." What Timiryazev once dreamed of, I. V. Michurin has achieved. He has discovered these new laws of development of life and has worked out the methods for obtaining directed variations in the nature of plants. Knowing the laws of plant development, I. V. Michurin revealed his genius by "moulding" new organic forms, and, moreover, forms that are needed by and are beneficial to man.

As is known, Darwin, to express it in the words of Engels, "leaves out of account the *causes* which have produced the variations in separate individuals."[1] In this he proceeded from the fact that "man does not actually produce variability," he is capable only of utilizing and accumulating by selection the variations provided by nature.

Michurin, however, making full use of Darwin's great theory of evolution, studied the causes of individual variations in organisms, discovered the laws that govern the development of plants and worked out methods of causing directed variations.

The teachings of I. V. Michurin, while constructively de-

[1] F. Engels, *Herrn Eugen Dührings Umwälzung der Wissenschaft*, Moskau 1946, S. 83.

veloping the materialistic core of Darwinism, are at the same time profoundly dialectical. I. V. Michurin proceeded from the fact that the organism's historical past is the foundation upon which alone its present and future can develop. He proceeded from the unity between phylogenesis and ontogenesis. He revealed the true interrelation of the organism's historical past and its hereditary basis.

I. V. Michurin criticized and refuted the principle, so characteristic of Mendelism-Morganism, that there is a rupture between the phenotype and genotype, between ontogenesis and phylogenesis.

Michurin regarded the organism as being inseverably connected with its environment. He revealed the extremely important part environment plays in the formation of the organism, and analyzed the different stages of this process of formation.

In his notes on "External Environment" (dedicated to the margarine wiseacres) I. V. Michurin wrote: "As is evident, some people who imagine they are scientific experts on the laws of the vegetable kingdom, naively throw doubt upon my statements about the influence environment exercises upon the process of production of new forms and species because, they claim, they have not yet been proved by science.

"... when thinking about these alleged scientists, one does not know what to be surprised at most—either their extreme shortsightedness, or utter ignorance and lack of all sense of their world outlook.

"First of all, it would be interesting to know whether they really think that all the 300,000 different species of plants were created (without any environmental influence) only by means of the hereditary transmission of the characters of their parents.... Such an explanation would be utterly absurd. Surely, it cannot be supposed that the entire vegetable kingdom that now exists all over the world was gradually created in the course of tens of millions of years out of the first individual germs of living plant organisms by means of cross-pollination, without being influenced by environment, the conditions of which have changed so often, and so much, during the past centuries and milleniums...."[1]

[1] И. В. Мичурин, *Сочинения*, том III, стр. 255-56.

Michurin reverts to the role played by environment in a number of his other works. Thus, in an article entitled, "Bureaucracy in Science," he wrote: "I regard as utterly absurd the assertion that variations in the structure of species and races in the vegetable kingdom [took place] only as a result of the hereditary transmission of the characters of parents without an equivalent participation of the environmental factors. Let us assume, in conformity with the existing hypothesis, that in the course of the past hundreds of millions of years, the hereditary transmission of its characters to its offspring by each of the parents and the mixing of the two sexes had to give rise to variations which grew into entirely different species and genera; but how can [doubt] beset any sound-minded observer when he sees before his eyes the influence constantly exercised upon the formation of plants by changes in external environmental conditions, composition of the soil and climatical changes? To have any doubts about the actual part played by these influences, one must be totally ignorant of the most elementary laws of life of every living organism. It is surprising how many times climatic conditions in all parts of the world have changed in the past; and to think that plants, adapting themselves to these changes in the struggle for existence, could have survived without changing their structure! This is too utterly absurd! No, such a fallacious idea is totally inadmissible.

"Only the joint action of the inheritance of ancestors' characters and of external environmental factors has created, and is continuing to create, all forms of living organisms. It is useless arguing against this indisputable truth." [1]

This is how I. V. Michurin presented the problem of the role of external environment. As we see, being a genuine dialectical materialist, in approaching the problem of the ways of directing plant organisms, I. V. Michurin combines the historical past of the organism's development with the role played by external environment.

He took into account the organism's historical path of development and also how on this path the organism adapted itself to the conditions of its existence. The mentor theory that

[1] И. В. Мичурин, *Сочинения*, том I, стр. 483.

he worked out is one of the outstanding achievements of science.

Dealing with the problem of environment, I must dwell for a moment on the question of Lamarck.

The Mendelist-Morganists are using Lamarck as a bogey; they have converted his name into a word of abuse. It is enough to say "this is Lamarckism" to make them fly like the Devil from holy water. The actual, scientific truth about Lamarck, however, is the following.

As a matter of fact, Lamarck was the first man in the history of the development of science to expound the theory of evolution. He laid down the proposition that the correct classification of organisms is the reflection of the order and evolution of organisms from each other. He pointed to the decisive influence exercised by environment on the development of organisms, proceeding from the proposition that it is not form that conditions the functions of the organism, but, on the contrary, functions, directed by the influence of environment, that condition form.

As is known, Lamarck's theory arose in connection with the ideas of the French encyclopaedists and the French materialists. It reflected the revolutionary epoch of that time. It possessed philosophical content, and was distinguished for its materialistic character. The reaction against the French Revolution also caused a strong reaction against the ideas of Lamarck, and this reaction continued throughout the nineteenth century.

Commenting on the role Lamarck played in the development of the theory of evolution, K. A. Timiryazev wrote: "*The Philosophy of Zoology,* where for the first time the question was raised from the scientific point of view as to whether all the presently existing organisms could not have evolved from each other in the course of time by a gradual and slow process of alteration." In another place he wrote: "Sober-minded Darwinism alone allocates to Lamarckism its proper place in science."

That is how the matter stands with Lamarck.

It must be observed that Darwin himself admitted his error in underrating the influence environment exercises upon the organism. In his letter to Wagner (1876) he wrote:

". . . the greatest error which I have committed, has been

not allowing sufficient weight to the direct action of the environment, i.e., food, climate, etc., independently of natural selection."[1]

Darwin gave a truly scientific explanation of the process of evolution. That is the immortal service he rendered to science, to mankind. But many decades have passed since Darwin's time. Science and life have accumulated a vast number of new phenomena and facts. People like B. M. Zavadovsky, who call themselves "orthodox Darwinists," evidently wish to say that they adhere to Darwin's theory without any modification, i.e., adhere also to his errors (elements of Malthusianism, gradual evolution, repudiation of leaps in variation, etc.), to his obsolete propositions, and refuse to go further forward.

Let them cling to this "general" line. Life and science will pass them and forge ahead.

The Michurin trend in biology represents a qualitatively new and higher stage in the development of Darwinism. After utilizing all the treasures of Darwinism, all that was best in Darwin's theory, I. V. Michurin took an enormous creative step forward in developing this theory.

Darwin laid the basis for the scientific explanation of evolution; I. V. Michurin made evolution. Herein lies the new feature, the gigantic step forward in developing Darwinism that characterizes the Michurin trend.

We can be proud of the fact that our fellow countryman, the great scientist I. V. Michurin, opened a new stage in the development of the science of biology, blazed new paths in this field.

We can be proud of the fact that our Soviet scientist I. V. Michurin discovered and mastered the laws of consciously directed development of organisms. Let all the cosmopolitans in science say that "questions of priority in science are of no importance." We, however, can legitimately be proud of the fact that this great contribution to biology was made by a Russian, Soviet scientist.

The Michurin trend in biology opens up new and boundless prospects for the science of biology, particularly

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[1] *The Life and Letters of Ch. Darwin*, Vol. III, London 1888, p. 159.

under the conditions that prevail in our country, under the kolkhoz system. Knowing the laws that govern the alteration of the heredity of organisms, and skilfully utilizing them, our scientists will work successfully to improve existing and produce the new forms of plants and breeds of animals that our socialist country, that Communism needs.

Michurin dreamed of transforming our country into a vast, flourishing garden. This dream is being realized in the land where Stalin's five-year plans are operating.

All those who oppose the progressive, truly scientific, Michurin trend in biology will be swept off the path of the fruitful development of our advanced science of biology.

Michurin's theory is a striking illustration of the application of the method of materialist dialectics in scientific research. I. V. Michurin himself highly evaluated the philosophy of dialectical materialism.

Listen to the inspired words he uttered on the philosophy of dialectical materialism:

"Science, and its concrete branch—natural science—in particular, is inseverably bound up with philosophy; but in view of the fact that man's world outlook manifests itself in philosophy, it is a weapon in the class struggle."

Further on Michurin speaks of the Party principle in science:

"Partisanship in philosophy is the chief orientating factor. The structure of things determines the structure of ideas. The progressive class, as the proletariat has proved itself to be, is the vehicle of a more progressive ideology, and works out a single and consistent Marxist philosophy.

"By its very nature, natural science is materialistic, materialism and its roots lie in nature. Natural science spontaneously gravitates towards dialectics. To understand the problems of natural science properly one must understand the only true philosophy—the philosophy of dialectical materialism."[1]

And lastly, I will quote one more statement by I. V. Michurin:

"Only on the basis of the teachings of Marx, Engels, Lenin and Stalin can science be fully reconstructed. The objective world—nature—is primary; man is part of nature, but he

[1] И. В. Мичурин, *Сочинения*, том I, стр. 446-47.

must not merely outwardly contemplate this nature, he can, as Karl Marx said, change it. The philosophy of dialectical materialism is an instrument for changing this objective world; it teaches how to influence this nature and to change it; but the proletariat alone is capable of consistently and actively influencing and changing nature—this is what the teachings of Marx, Engels, Lenin and Stalin—those unexcelled titanic minds—tell us." [1]

That is how I. V. Michurin linked his theory with the philosophy of dialectical materialism. So clearly did he perceive the inherent connection that exists between his effective theory of directed development of plant organisms and the effective philosophy of dialectical materialism.

Only that science deserves the name of true science which leads to the discovery of the laws of development of the world by which man is surrounded, which reveals new horizons and prospects to man. Only that science deserves the name of true science which serves practice, which is tested by practice, and produces tangible results in life. This is precisely what characterizes the Michurin trend in science.

The future belongs to the Michurin trend. V. I. Lenin and J. V. Stalin gave the highest appraisal of the theoretical works and practical results achieved by I. V. Michurin by calling him the great transformer of nature. That is why the Soviet Government, our great Bolshevik Party and Comrade Stalin himself in every way support and foster the Michurin trend in science.

The followers of Mendelism-Morganism have been warned more than once that their trend in biology is alien to Soviet science, that it leads to a dead end. It is worth recalling some of the stages of this struggle. In 1931 the Party censured Menshevistic idealism in philosophy and natural science. This directly concerned men like Professor Serebrovsky, Dubinin, and others. The decisions of the Party and other public organizations at the Institute of Philosophy and Natural Science, pointed to the following extremely grave errors committed by a number of persons in the fields of biology and physics: "adoption of the standpoint of autogenesis," "Machist

[1] И. В. Мичурин, *Сочинения*, том I, стр. 447.

utterances in the field of physics and mathematics," and others. [1]

In 1939, during the wide discussion on genetics organized by the editors of *Pod Znamenem Marxisma,* the Mendelist-Morganists received a severe rebuff, and the theory of the gene, the divorcement of the formal geneticists from practice, etc., were criticized. Nevertheless, far from drawing the proper lesson from this, the Mendelist-Morganists continued to advocate and elaborate their fallacious ideas.

The inordinately protracted controversy, and the active propaganda of their views by the Mendelist-Morganists, are having a gravely harmful effect upon the work of ideologically training our cadres. The main purpose of this session should be to put an end, at last, to this inordinately protracted controversy, to expose and demolish the unscientific conceptions of the Mendelist-Morganists and thereby lay the foundations for the further development of Michurin's researches, for further success of the Michurin trend in biology.

The many years of struggle between the two trends in biology have irrefutably proved that the Mendelist-Morganist trend in biology is reactionary and inimical to the interests of the people, that it is retarding the further development of the science of biology and is causing grave damage to practical, socialist agriculture. They have also proved that the Michurin trend is the most progressive trend in biology, a trend that is promoting the further development of the theory of evolution, is equipping the practical workers in agriculture with effective, scientific methods of improving the old and producing new varieties of plants and breeds of animals. The measures that are being taken in our country to develop plant breeding and seed cultivation, to increase the fertility of our socialist fields, to plant shelter belts, to combat drought, etc., will be the more successful, the more profoundly and many-sidedly the Michurin theory is developed.

The future belongs to the Michurin trend in biology. This imposes upon all truly progressive Soviet biologists, geneticists and breeders the obligation to intensify their study of the precious theoretical heritage left by I. V. Michurin, to intensify

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[1] Сборник *За поворот на философском фронте*, 1931 г., стр. 231.

their study of the urgent problems of biology and thereby take a still more active part in carrying out the historic tasks our Party and our Government have set us in order to raise our socialist agriculture to a still higher level.

At this session note must be taken of the part Academician T. D. Lysenko has played in the struggle the progressive trend in biology has been waging against the reactionary trend. There is no time just now to deal with the most fruitful theoretical and practical results in developing the Michurin trend that are associated with the name of Academician T. D. Lysenko. There is no time to speak of his theory of the phasic development of plants, which is a tremendous theoretical achievement in the field of biology, of his views on the problems of heredity, and on other extremely important problems of biology. A special address would be required for that. At present I want merely to note the following. Boldly and resolutely, with the indomitableness and passion that are characteristic of him, T. D. Lysenko has been and is exposing Mendelism-Morganism. He was obliged to overcome immense difficulties, he was slandered, it was said that he is "unscientific," innumerable obstacles were placed in his way, but he boldly pushed forward like a true innovator in science, ignoring everything, militantly vindicated his principles and held aloft the banner of the Michurin trend. Academician T. D. Lysenko—the Michurin of our times—has made an immense contribution to the development of biology and to practical, socialist agriculture. I think I will be expressing the opinion of the overwhelming majority of those present here if I say that thanks to the courageous and fearless struggle Academician T. D. Lysenko has waged against the conservatives in science, the Michurin trend in biology has been further developed, and such important successes in our science of agrobiology have been achieved. (*Prolonged applause.*)

Under the leadership of our Bolshevik Party, the majestic process of building Communism is taking place in our country. Communism is our joyous and not distant future. In this militant task of building Communist society, our scientists, innovators in science and practice, are playing an honourable part.

Under the leadership of the greatest genius of modern

times, our dear and beloved teacher Comrade Stalin, Soviet science, our scientific innovators, will achieve even greater results and successes. (*Loud applause.*)

Academician P. P. Lobanov. I call upon Comrade E. M. Chekmenev.

E. M. Chekmenev (Deputy Minister of State Farms of the U.S.S.R.). Comrades! The subject of our discussions here, namely, the condition of biological science in our country, is of immense scientific and practical importance.

Only in our Soviet Socialist State, where the development of science is the daily concern of the Communist Party and of its great leader, Comrade Stalin, do the existing conditions provide the possibilities for creative work on the scale we witness today.

Our science is the most advanced, most progressive science in the world. This is because it is based on dialectical materialism, on the scientific revolutionary theory of Marx-Engels-Lenin-Stalin.

The aim of the fruitful activity of our scientists is to serve the people, our Soviet State. Therein lies the enormous advantage possessed by our science over that of any capitalist state, where it serves the mercenary interests of private profiteers, and is a means of enriching the exploiting classes, and oppressing the working people.

Pseudoscientific formal genetics, as made clear in the address by Academician Lysenko, is used by the ruling classes of capitalist countries to prop up their metaphysical and idealistic notions.

Comrade Rapoport, whom we heard yesterday speak from this rostrum, exhorted us to be "humble" in our claims, to confess "our errors." He said that all that is best in science, whether established inside or outside this country, must be turned to account in the interest of further scientific progress.

But he manoeuvred on the main issue, and left it unanswered. The question is what science is to provide the principles according to which we generalize all the data we accumulate? Shall it be the principles of reactionary Weismann genetics or the principles of truly scientific biology, the biology of Sechenov, Mechnikov, Timiryazev, Williams, Michurin and Lysenko?

As a matter of fact there are two world outlooks in biological science: The materialistic outlook, on which genuine science, Michurin biology, is based, and the bourgeois idealistic outlook on which Mendelism-Morganism is based.

The advocates of Mendelism-Morganism in this country have committed themselves to the reactionary Weismann genetics. There is no such thing as non-partisan science. That has been proved long ago. Therefore Comrade Rapoport cannot possibly succeed in convincing us that Michurin biology and reactionary biology can live in peace together. Of course they cannot, because Michurin biology is a science based on Party principles and cannot permit of a conciliatory attitude to reactionary biology.

The Michurin trend, headed by Academician Lysenko, which is developing the evolutionary theory of Darwin, Timiryazev and Michurin has made substantial progress both in the field of theory and of practice, and has supplied us with knowledge which has been applied extensively to our socialist agriculture. Thanks in part to it we have secured higher harvests and improved our animal industry.

In 1943 Lysenko published his book, *Heredity and Its Variability,* in which he expounded the theoretical principles of Michurin genetics. This work is the direct continuation and further development of the theories of Timiryazev and Williams.

Timiryazev confronted science with the boldly conceived problem of "moulding organic forms," and himself did much to secure its successful solution. The generalizations made by Timiryazev in the sphere of heredity and the work of Michurin on the directed modelling of the inherent qualities of the hybrid seedlings of wood and shrub species made it possible for a much bolder approach to be made to the study of the laws of variation.

The investigations of Michurin and Lysenko on sexual and vegetative hybridization disclosed the point that the source of hereditary variation lies in continuous metabolism.

Thus the trend initiated in biology by Michurin is important because it enables us to master the process of consciously directing heredity, that most complicated phenomenon of life.

Academician Lysenko and his pupils have enriched agriculture with a number of most valuable agrotechnical methods

such as the vernalization of cereals, potatoes, beets and other farm crops, and as the theory of the directed alteration of winter plants to spring plants, and of spring plants to winter plants, the summer planting of potatoes in the South, the summer planting of alfalfa, etc.

The planting of alfalfa for seed in the summer, as suggested by Lysenko, has been extensively practised at sovkhozes. This year vast areas have been thus planted with alfalfa and esparto grass. Practical experience had convinced sovkhoz employees in the southern regions how exceptionally effective are summer plantings as a means of securing high and stable yields of seed.

The Ministry of State Farms has undertaken a series of measures intended radically to solve the problem of seed-growing for perennial grasses. One of the foremost of these measures is the summer planting of alfalfa.

There has also been an extensive application at sovkhozes of such agrotechnical methods as the summer planting of potatoes in the South, the topping of cotton plants, the added artificial pollination of agricultural plants, the use of warm air to stimulate seeds at sovkhozes in Siberia, a method that has proved most effective in increasing the germinating power of seeds, and so on.

The different varieties of farm crops obtained on the basis of the Michurin theory are extensively employed. At the sovkhozes alone more than 100,000 hectares will this year be put under such splendid varieties of winter wheat as Novo-Ukrainka 83, Krasnodarka, Pervenets, products of the Krasnodar State Plant-Breeding Station.

Hundreds of thousands of sovkhoz employees have put Lysenko's agronomical discoveries to a practical test. In no other country has any scientific discovery undergone, or could it have undergone, the test of practice on such a mass scale as have the discoveries made by Academician Lysenko and other Michurinists.

No breeding work at sovkhozes, whether with plants or animals, no seed-growing work can now be thought of on any other basis but that of the theory of Michurin.

At our sovkhozes all the practical work for further improving plants and animals and for producing new varieties and

new breeds is governed by Lysenko's statement that "Good varieties of plants or animals are always produced only by the application of proper methods of cultivation or breeding. Under poor cultivation no good strains can ever be produced out of poor ones, and in many cases even good strains will deteriorate after a few generations."

The theoretical views of the school of formal genetics, far from being a help to our socialist agriculture, actually hinder its development.

In animal husbandry the Morganist-Mendelists have artificially separated problems concerned with environment and the development of animals from those concerned with breeding, and have at bottom reduced the problem of selection and breeding work to appraising sires by the quality of their offspring, to searching for "leaders" in a given breed, for "lethal" genes, etc.

Proceeding from their fallacious theoretical standpoint the Morganist-Mendelists have established the erroneous method of appraising sires by individual characters. According to this method a sire that fails to correspond in some decisive quality to the type bred for may, if it shows positive features in a number of characters of secondary importance, obtain a good evaluation and be widely used in the herd, to the detriment of the main object being pursued.

The theoretical principles of Mendelism-Morganism when applied to practice guide it along wrong channels and cause serious economic damage.

In support of this assertion I should like to give you the following example from the practical experience of animal breeding in our country. As is well known both the Party and the Government pay tremendous attention to problems of animal breeding, being concerned to increase the output of the animal industry to the utmost, in particular by improving our breeds as rapidly as possible.

Accordingly a vast network of state pedigree breeding farms and breeding stations has been established in this country, where the breed crossing and breed improvement of cattle is practised on a mass scale. Our Mendelist-Morganists, however, have made a negative contribution to this great work of effecting a radical improvement in the quality of our livestock.

Freely admitted to participation in organizing breeding work on the state pedigree breeding farms and breeding stations the Mendelist-Morganists based their work on the chromosome "theory" of heredity, which is an idealistic theory that postulates the continuity of the germ-plasm. In accordance with this "theory" every state or collective stock farm was required simply to repeat the superior breed, to accumulate "pure bloods" and "pure lines" from superior breed, without regard to the productivity of the animals thus obtained. The active role of the environment in animal breeding was denied.

According to these principles the zootechnicians were in fact obliged to discard even the most productive animals from the herd on the mere ground that they failed to correspond to the requirements of formal genetics even in some character of secondary importance. Work with half-breeds and grades was forbidden, and indeed, from the point of view of formal genetics, there was no sense in doing such work.

It should be stated now that this trend in stockbreeding seriously delayed the improvement of our herds. Grades of higher generations lost a number of economically valuable properties of local breed because they were merely bred for identity with the superior parent. This hindered the formation of new highly productive breeds of cattle to meet the growing requirements of our socialist economy, and ruled out any creative activity on the part of experts, collective farmers and sovkhoz workers in the sphere of breeding work.

Due to the chromosome "theory" of heredity, which held the field at that time, even the best results obtained by our Russian breeders who did not share the views of the Morganist-Mendelists, were either passed by in silence or strongly criticized.

Such was the fate of the work done by M. F. Ivanov, who succeeded in creating a remarkable home breed of fine-wooled Askania sheep. When it was no longer possible to ignore the work of Ivanov, derogatory comments were made intended to diminish its importance. His success was ascribed to "pure accident," to his "peculiar intuition," or explained away in some other manner. The idea that his method might be of theoretical value was denied. Professor A. S. Serebrovsky

at a meeting of the Academy in 1935 flatly declared that he expected his own method to yield far better results.

The Morganist-Mendelists had their way at the time, and the highly valuable flock of Askania sheep was attested as being not a new home breed, but merely a type of the foreign Rambouillet breed.

As for Serebrovsky's methods, they proved a failure when tried out on the Kotovsky State Sheep-Breeding Farm, Stalingrad Region. Breeding work was started there in 1933, and has been continued up to the present by Serebrovsky's pupil and follower J. L. Glembotsky, but without success. Fifteen years of work have not yielded any improvements in the quality of the sheep. Thus the average fleece in 1947 weighed 3.2 kg. as against 3.1 kg. in 1934. The average live weight of élite dams was 48.7 kg. against 49 kg. in 1933. Such are the practical "results" of long years of the application of the Mendelist-Morganist theory.

At the same time it should be mentioned that in 1938 the Academy approved a draft program for regional breeds, based on the theoretical principles of the Morganist-Mendelists. The program provided for the remodelling of our livestock into a number of breeds bearing foreign names.

The work of our true scientists, the Michurinist zootechnicians, and the experience gained at our foremost sovkhozes and kolkhozes utterly refuted the method of organizing breeding work and mass upgrading on the basis of the "chromosome theory" of heredity, and showed how useless and harmful it was. The methods of Michurin genetics then began to be extensively employed in cattle breeding.

The results soon became apparent, and we are now witnessing the creation of home breeds of cattle, sheep, pigs and horses, breeds in every respect superior to any foreign breed.

This may be exemplified by the work of K. D. Filyansky, Stalin Prize winner, who has produced a new breed of Caucasian Rambouillet at the Bolshevik State Farm.

His work was conducted on the following principles:

1. A clear definition and characterization to be given of the type of sheep desired and fitting in most completely with the economic conditions of the given sovkhoz, the require-

ments of the national economy, and the biological features of the initial animals.

2. Optimal conditions to be created for caring for and feeding the young and for raising highly productive animals.

3. A proper selection of parental pairs and a systematic test to be made of the rams as regards the quality of their progeny, extensive use to be made of the best sires, line breeding—the best lines to be crossed with one another.

In testing the rams as regards the quality of their young, Filyansky rejected the method of evaluating individual characters as suggested by the Morganist-Mendelists. Instead he based his evaluation on a complex index, which referred the young to a certain class, and showed how far they corresponded to the type bred for, account being taken of their constitutional characters.

It was not long before he obtained and was able to fix the type of animal he was breeding for. As a result, the Bolshevik State Farm now has a highly productive flock, with an average fleece of about 6.0 kg. But as a result of the pressure of the Morganist-Mendelists this valuable flock was also attested as being merely a Caucasian type of the foreign Rambouillet breed, and not as a new home breed of fine-wooled sheep.

Another example of the good results obtained by employing progressive Michurinist methods, is to be seen on the Sovetskoye Runo State Farm, where the zootechnician S. F. Pastukhov has succeeded in creating a large flock of homebred fine-wooled sheep, which are both highly productive and remarkable for the valuable technological qualities of their wool, a very long, thin, silky and strong wool indeed.

This flock, numbering many thousand animals, ranks among the best sheep flocks of the U.S.S.R.

But what was the attitude of the representatives of formal genetics to these outstanding achievements?

Take, for instance, the book by J. L. Glembotsky, Y. K. Deichman and G. A. Okulichev, entitled *The Stock-Breeding of Fine-Wool Sheep.* There we have the following passage: "Wherever the classing was done systematically by experienced people with a thorough knowledge of the exterior of the sheep and their wool, the result was invariably a considerable

increase in the productivity of the flock, and occasionally new breeds or types of sheep were produced. As a recent example of that kind, we may mention the results obtained in the Ipatovsky No. 22 and Sovetskoye Runo No. 11 state breeding farms. There, selection carried on consistently for fifteen years, mainly in respect of the phenotype, has produced splendid flocks of Caucasian Rambouillet, which are to be classed among the best fine-wooled sheep flocks of the U.S.S.R. Nevertheless, we cannot consider this method of selection to be a perfect one because it is based on the conception of the complete identity between phenotype and genotype. Modern genetics has shown that this conception is wrong in principle. . . ."[1]

All one can say is that if the best flock of fine-wooled sheep in the U.S.S.R. is the result of the application of principles that are wrong from the viewpoint of modern genetics, so much the worse for this so-called "modern genetics."

One could cite many other examples of the same effect. Take, for example, Balmont's achievement in creating a Kazakh fine-wooled breed of sheep, and the results obtained by G. R. Litovchenko and N. A. Vasiliev with a Siberian breed of fine-wooled sheep, and also other examples, which provide incontrovertible proof of what can be attained by giving practical application to the progressive theory of Michurin-Lysenko.

Is there anyone in this country who does not know the outstanding achievements of the Stalin Prize winner S. I. Shteiman, senior zootechnician of the Karavayevo State Breeding Farm? As a result of long years of selection and breeding he succeeded in creating a breed of cattle that is second to none in the world as regards productivity and other valuable qualities. This is a new Kostroma breed.

Just what are the principles adopted by Shteiman in his work? They are the same as Michurin applied in plant breeding, and which are employed by all breeders who base their work on Michurin's theory.

"These principles," Shteiman tells us, "may be boiled down to the following: proper raising of the young, adequate feeding,

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[1] Я. Л. Глембоцкий, Е. К. Дейхман и Г. А. Окуличев, *Племенное дело в тонкорунном овцеводстве*, Москва 1947 г., стр. 163.

due care, skilful selection and suitable pairing of parents. It must be emphasized that all these zootechnical measures are to be applied, not separately but in strict and daily combination.

"Breeding will not be effective unless a congenial environment is created for the animals, which means that exemplary arrangements must be made for keeping, caring for and feeding them."

It will be obvious from this that the principal factor here is man. His ability to direct the heredity of organisms by controlling the living conditions of these organisms decides the results, which means that the main part is played by Michurin genetics, and not by the Mendelists with their continuity of the germ-plasm.

Comrade Shteiman and the group of progressive farm workers taught by him at the Karavayevo State Farm have raised a herd of cows with an average annual yield of milk per cow for the decade 1937-1946 amounting to 5,660 kg. The average yield for a herd of 250 cows was 6,310 kg. in 1940, and for a group of 88 cows in 1947 was as high as 6,616 kg.

Highly valuable family-lines have also been bred. The line from Belyana gives an annual milk yield in the progeny of 7,739 kg., containing 3.74% of fat; the line from Poslushnitsa Pervaya, 7,920 kg. with 3.72% of fat; the line of Simpatia, 6,943 kg. with 3.90% of fat.

Individual cows raised at this farm—Opytnitsa, Lenivaya, Katya, Blagodat—gave life yields of more than 100,000 kg. of milk. These are world records.

The herd of this farm is remarkable for high live weight, strong constitution, vigour, and adaptability to local conditions. The Government has shown high appreciation of the work of the employees of the Karavayevo farm. The Kostroma breed of cattle is undoubtedly one of the best in the world as regards productivity. Shteiman's rich experience will serve as a useful guide for all sovkhoz employees engaged in improving our livestock and in increasing animal productivity.

Convincing as are the outstanding results obtained by our practical breeders, they have made no impression on the advocates of formal genetics, who refuse to recognize the new breeds of farm animals on the ground that they have been

produced in defiance of their theoretical postulates, and do not fit in with their wrong views on breeding.

One could cite many more instances of the fruitful activity of the followers of Michurin biology in the field of stock raising, but those I have mentioned will suffice. It may be positively stated that directed breeding is becoming a law for those occupied both in plant breeding and in stock raising.

Michurin genetics has worked out the theoretical principles for animal breeding and mass grading at sovkhozes and kolkhozes. These principles will be a law, a *sine qua non* for all practical workers.

Comparing the great creative achievements of the followers of Michurin, who have been developing agrobiological science in close contact with practice, and the endless marking of time by the conservative formal geneticists, whose work is detached from practice, we may well recall the following words of Chernyshevsky, the great Russian revolutionary democrat: "Only those trends of human activity achieve brilliant development that are bound up in the closest degree with the requirements of society. That which has no root in the soil of life, remains dull and colourless, and is not only devoid of historical importance, but regardless of its effect on society is, by its very nature, of no significance."

There is no doubt in our minds, comrades, that in our country agrobiological science will develop along the lines laid down by Timiryazev, Michurin and Williams, and this will lead our science to still greater discoveries, thus ensuring the further progress of socialist agriculture.

"Life has changed. It is now full of purpose, has become interesting and joyous. Therefore," says Michurin, "both plants and animals must be raised to a higher level of productivity and vigour so as to correspond more closely to the requirements of our new life. This is only possible on the basis of all-powerful technique, on the basis of all-powerful selection."

There is no doubt in our minds that in our country agrobiological science will develop on the lines laid down by Timiryazev, Michurin, Williams, Lysenko. The biological science of Michurin is strong because it strictly follows the words of Stalin. It "does not fence itself off from the people and does not hold aloof from them, but ... is prepared to serve the

people and to transmit to them all the benefits of science, and ... does not serve the people under compulsion, but voluntarily and willingly."[1] For this, great credit is due to the Michurinists. We wish them further progress for the benefit of our Motherland. (*Applause.*)

Academician P. P. Lobanov. I call upon Comrade A. V. Pukhalsky.

A. V. Pukhalsky (Deputy Chief of the Central Grain and Oil-Bearing Crops Administration, Ministry of Agriculture of the U.S.S.R.). Comrade Stalin teaches us that "...theory, if it is genuine theory, gives practical workers the power of orientation, clarity of perspective, confidence in their work, faith in the victory of our cause."

It is on this basis that we must consider and evaluate the work of our scientists.

Practice, as is well known, is the sole criterion in testing theory.

For us, workers of the Ministry of Agriculture, who are called upon to solve the practical problems of the socialist farming industry, the suggestions of scientists aimed at increasing the yield and quality of farm crops are highly valuable.

There have been instances where wrong theories have hampered the development of our socialist farming. On the other hand, effective theories based on the teachings of Marx-Engels-Lenin-Stalin have resulted and continue to result in practical progress, and are of assistance to our practical workers. We are not indifferent as to what trend our scientists follow in science. The success of our work depends on that.

The trend initiated by Michurin in agricultural science regards all natural phenomena in their interaction. New varieties of agricultural plants are considered as a result of the development of organisms in a given environment. This point of view equips the practical worker and facilitates the creation of new varieties, increases the yield of agricultural plants and raises efficiency in agriculture as a whole. On the other hand,

[1] Сталин, Речь на приеме в Кремле работников высшей школы 17/V 1938 г.

the mechanistic way of analyzing natural phenomena, which ignores the influence of the environment on the development of organisms, renders the practical worker helpless because actually it excludes the possibility of development and reduces all new formations to the recombination of hitherto existing forms, or ascribes them to casual mutations.

The followers of Michurin have enriched agriculture with valuable varieties of fruit plants, vegetables, cereals and technical crops. They have developed new agrotechnical methods for increasing and stabilizing harvests.

On the other hand, no valuable results have been secured for practical agriculture by works based on the Morgan-Mendel theory. We can hardly expect that the cabbage-radish hybrid, or the amphidiploids of wheats, which, by the way, have not passed the test-tube stage for many years, or indeed the available amount of *Drosophila* breeds, however extensive, will be of any importance in increasing the harvests on the fields of our kolkhozes and sovkhozes.

The investigations made by Michurinists in the sphere of agrobiological science, under the guidance of Academician T. D. Lysenko, are of immense importance to us because they are effective, because the methods of breeding and the agrotechnical measures evolved on their basis yield positive results, and, again, because they have disclosed the pseudoscientific character of the Morgan-Mendel theories and advanced our Soviet agricultural science.

While working in the field of theory the followers of Michurin are mindful of the requirements of practice, and do not consider their investigations complete unless they have received wide application at sovkhozes and kolkhozes. In that respect our Michurinists follow the advice of Timiryazev who used to say that "it is not enough to cast a happy thought to the world. What is primarily needed is that it assume the form of an irrefutable fact."

Everybody knows about vernalization, the agrotechnical method evolved by Lysenko on the basis of the theory of the phasic development of plants. Before this method could be developed it was necessary to abandon the generally recognized approach to the study of factors responsible for the variation of organisms. The former approach was that these factors must

be sought only inside of the organism, while the influence of the environment on its development was entirely neglected. In vain did the formal geneticists try to explain the length of the vegetative period in plants, their spring or winter character by the presence in their cells of "heredity carriers," the polymeric genes.

Not until he had adopted a fundamentally different approach to the problem by developing Michurin genetics, did Lysenko succeed in properly explaining the nature of the growth period in plants, and reveal the role of external conditions in the formation of organisms, in their heredity and variability. He was the first in the world to do this.

After the nature of the vegetative period had been correctly, scientifically interpreted on the basis of the theory of phasic development, it was possible to suggest to the practical worker a number of agrotechnical methods for increasing the yield. Among these methods are those of checking the degeneration of the potato in the South, the remodelling of plants in the desired direction, accelerated breeding of new highly productive varieties, and many other valuable agricultural procedures.

That the vernalization of cereal seeds as a means of increasing the yield is now extensively used at sovkhozes and kolkhozes is a matter of common knowledge. It will suffice to say that far from complete data show that in 1940 the kolkhozes of this country planted over 14,000,000 hectares with vernalized seed. At present the vernalization of cereals is again being extensively employed by kolkhozes. The program for 1948 provided for the planting of vernalized seed over an area of 6,900,000 hectares.

In 1947 at the February Plenary Session of the Central Committee of the Communist Party of the Soviet Union which discussed the problems facing agriculture in the postwar period, it was decided that courses for training vernalization experts must be organized among collective farmers in order that beginning with 1948 the planting of cereals and millet might be done with vernalized seeds in those regions where this procedure was employed before the war.

The vernalization of potatoes is likewise widely employed at sovkhozes and kolkhozes.

It was on the basis of the theory of phasic development

that the causes of the degeneration of the potato in the South were discovered and the summer planting of the potato recommended as a means of combatting this phenomenon. The summer planting of the potato is also a graphic example of the great importance of the environment in the development of organisms. The investigation of this problem under the guidance of Academician Lysenko at the All-Union Institute of Selection and Genetics in Odessa has literally saved the potato-growing industry in the South by relieving it of the necessity of bringing in seed potatoes every year from the northern and central regions of the country and by sharply increasing the yield of this crop.

For the first time in the history of agriculture the nature of plants is being remodelled according to a pre-arranged plan by means of training. By studying the requirements of plants in respect to external conditions, requirements determined by their phylogenetic development, and by steadily altering these conditions and thereby bringing the desired influence to bear on the development of the organism, Lysenko has been able to show the possibility of modifying the nature of plants by means of training. Spring varieties of wheat can be remodelled into winter varieties and vice versa. Thus the spring variety No. 1163, after being transformed by training into a winter variety, perfectly well withstands the Siberian winter. Investigations into the remodelling of plants by training are now being successfully conducted by the Siberian Scientific Research Institute of Grain Husbandry, the All-Union Institute of Selection and Genetics, and other research institutions.

All this, as is well known, is incompatible with the Mendel-Morgan theory, which asserts that the development of an organism is entirely determined by its inherent qualities.

Experiments on the remodelling of plants fully confirm the prime role played by the environment in the development of organisms and in heredity and variability. The possibility of obtaining by means of training new hereditarily stable varieties with desirable qualities has been proved by experiment. Thus the breeder has obtained a new effective method for improving the varieties of agricultural crops.

Late autumn sowing of spring wheats on stubble fields in Siberia yields more productive seeds for sowing. Seed crop se-

cured this way is 3-4 c. per ha. in excess of yields from spring sowings. What is more, the method of stubble ground sowing is useful for remodelling plants. By this means highly winter-hardy winter varieties can be obtained, and this acquired quality can be transmitted by heredity to subsequent generations.

The knowledge secured of the nature of organisms resulting from the application of Michurinist genetics provides our breeders with an effective method of working. Mendelism-Morganism, on the contrary, acts as a brake on the development of science in our country.

A fact known to you all is that Johannsen's theory of "pure lines" was long dominant among our breeders and geneticists. A "pure line" was considered to be invariable, and attempts made by some breeders to obtain new varieties from these "pure-line" varieties were regarded as unscientific. Academician Lysenko, however, has proved that "pure lines" are variable, and are apt to degenerate if the variety is kept for long under conditions of self-pollination.

The method of intravarietal crossing of self-pollinating plants, proposed by Lysenko, has made it possible for the breeder to increase the yield of varieties and to make them more viable and more resistant to unfavourable external conditions. This method, based on the theory of Darwin, Michurin, Lysenko that prolonged self-pollination is harmful to the organism of the plant, while cross-pollination is beneficial, utterly disproved Johannsen's theory of "pure lines," and was a positive contribution to the work of improving varieties.

By means of intravarietal crossing our breeding stations raise more productive élite regionalized seed for sowing.

In his elaboration of Darwin's theory Academician Lysenko showed what damage can be done by close inbreeding of plants or animals. Accordingly, he proposed open intervarietal crossing as a method of producing new varieties. In practice we already know of wheat varieties obtained by this method. In the current year the Mironovo State Breeding Station has turned out for state varietal tests a highly productive variety of winter wheat produced in this way.

At present intervarietal crossing is one of the main methods employed by our plant-breeding stations.

Academician Lysenko revealed the nature of dormancy in seeds of agricultural plants and on this basis developed a method, of use to practical agriculturists, for increasing their germinating capacity and the intensity of the germination of plants. By heating them up with warm air and shovelling them, germinating power can at once be restored to tens of tons of seeds provided that they have not lost their viability. This eliminates the need for engaging in cumbrous operations connected with the exchange of seeds.

The heating of seeds was employed by collective farmers during the war, and in recent years has been applied on a particularly large scale at both sovkhozes and kolkhozes in the northern regions.

Of undoubted importance for practical breeding as well as for the study of the principles underlying the theory of heredity and variability is the vegetative hybridization of plants. The production of vegetative hybrids, i.e., of plants obtained in an asexual way, defies explanation on the basis of Mendelism-Morganism. Nevertheless this method enables breeders not only to govern the development of plants, but also to use it for the production of new desired varieties of cereals, vegetables, technical cultures, etc. A rye-wheat hybrid of high productivity and winter hardiness has already been obtained by vegetative hybridization at the Yaroslavl State Plant-Breeding Station.

According to Lysenko, there is no competition between the individual members of a given species. This idea has opened up a new approach to such agricultural problems as the cultivation of kok-saghyz (bunch planting), the growing of protective forest belts, etc.

Another important problem which I cannot refrain from touching on is the extension of winter-wheat growing into the steppe region of Siberia.

As is well known, many attempts by scientists and practical agriculturists to cultivate winter wheat in the steppes of Siberia ended in failure. The plants were injured by frost, and new seeds had to be brought again and again from the southern regions. The explanation given for the ruin of the winter wheat was the effect of the low temperatures. In vain did breeders attempt to create new frost-resistant varieties for this zone. The causes responsible for the inability of winter wheat to survive

the Siberian winter were revealed, however, when the problem was studied with due attention to the interrelations existing between plant and environment. The sowing of winter-wheat seeds on stubble land and the appropriate combination of agrotechnical measures proposed by Lysenko (proper choice of plots, sowing of the preceding culture—spring wheat—on fallow land, adding phosphate fertilizers before sowing the winter crop, the planting of winter wheat on stubble land and adding supplies of nitrogen fertilizers early in spring) will carry any variety of winter wheat through the winter in the Siberian steppe belt, and the harvests will be found to be high and stable.

The Michurin trend in science is a great help to our collective farmers and agronomists, who are thus equipped to wage a struggle for better yields and greater efficiency in agriculture.

On behalf of practical agriculturists I therefore declare myself in favour of developing the science that helps us to raise the yield, to increase the output of the agricultural industry, to make our country stronger and improve the well-being of our people. (*Applause.*)

Academician P. P. Lobanov. The session is adjourned till tomorrow.

SIXTH SITTING

Morning, August 4, 1948

Academician P. P. Lobanov. Let us continue our work. I call upon Comrade F. M. Zorin, Head of the Plant-Breeding Department of the Sochi Experimental Station.

F. M. Zorin. One of the participants of this conference has described the Sochi Experimental Station as the Siberia of the subtropics. This seems to be a most fitting comparison. The late Academician Boris Alexandrovich Keller called it the northern outpost of the subtropics, on account of its location at the extremity of the northern frontier of the possible extension of citrus cultures. This is why the work of this station is entirely devoted to problems connected with the cultivation of highly valuable subtropical plants and their extension farther North.

The staff of the station solves these problems by developing scientifically tested agrotechnical methods for cultivating these valuable cultures and breeding new varieties better adapted to local conditions.

We base our work on the methods evolved by that great transformer of nature, I. V. Michurin, and by his eminent continuator, Academician Lysenko.

The work of the Sochi Experimental Station in the abovementioned field, has been dealt with in sufficient detail in the press. Moreover, many of the participants of this conference have no doubt visited the station and acquainted themselves with our plants and experiments, when on vacation at Sochi. I shall therefore not stop to deal with a number of investigations which though important are sufficiently well known. I shall dwell briefly on certain methodical problems which we

have been able to solve in recent years while pursuing our main line of research.

Our experiments have been based on Michurin's mentor theory and Lysenko's theory on heredity and its variability. We have proceeded from the principle that it is unthinkable in our times to conduct theoretical or practical work in the field of plant breeding if it is not based on this theory of Lysenko.

I must say that in the outlying districts of our country Lysenko's *Agrobiology* has become so scarce, is such a rarity, that I think I shall make no mistake if I suggest that it is high time that this book, as well as other of Lysenko's writings, should be published in large editions.

Michurin's mentor method has been used by us in relation to the reproductive organs of plants, and in this way we have arrived at a new method of combining sexual and vegetative hybridization and have disclosed the possibility of obtaining hybrids from three and more different parental forms simultaneously.

Every living organism is known to build its body out of the elements that go to make up its environment, and does so in accordance with its nature, its heredity. By virtue of heredity organisms that grow up under the same environmental conditions are distinguished from one another by different qualities, by the degree of resistance to frost or drought, by the period of fruit ripening, external appearance, chemical composition, productivity and so on. Thus a number of highly valuable characters and qualities required for breeding work are not concentrated in a single plant but are rather scattered among various plants. The task of the breeder is to bring together in a single plant, or rather within a single variety of plant, as many useful characters as possible. The best way to achieve this is by combining sexual and vegetative hybridization.

Our method consists in fertilizing the flowers of one plant with the pollen of another, and grafting the pollinated flowers on a third plant.

The three plants (of which two are used as the initial form in sexual hybridization, and the third is used as a mentor) are so chosen as to possess different useful qualities.

The fruit that develops from the ovary of the grafted

flower will take up food from the elements supplied by the leaves and roots of the mentor.

If the components are aptly chosen the fruit may be altered externally beyond recognition by the influence of the mentor. We may say, altered externally and internally, since for us Michurinists, the change of external characters means a change of internal characters also. It may happen that the external characters of the fruit do not change, and that only the seeds appearing in the fruit do so. And it may happen again that neither the seed nor the fruit show any alteration of external characters. In that case the flowers formed on the plants grown from the seed of the grafted fruit will have to be grafted for a second time on the same mentor.

Our experiments are conducted with perennial fruit plants, whose seed-to-seed cycle extends over a number of years. Therefore it is hard to make a rapid check on whether the modifications thus induced by environmental action are or are not transmitted by heredity.

In order to clear up this question we also conducted experiments on annual plants. These experiments were conducted with two varieties of Phaseolus: Mont d'Or with black seeds, and Giant with green pods and yellow seeds. Buds from the black variety were grafted on the yellow one, and conversely. When the fruits formed it was found that the seeds in the pods were unaltered by either grafting. It appeared as if the representatives of the formal school in biology were right. The environmental action had no effect whatsoever. However, we made repeated graftings and found that in one case the yellow seeds were transformed into black ones, and in another the black seeds began to be transformed into yellow ones. I have brought these seeds to the conference as material evidence.

We planted the altered seeds without grafting and in two generations have obtained above three thousand seeds with altered heredity.

Similar experiments have been made by us on other plants, annual as well as perennial.

Flowers of the eggplant, for instance, were grafted on perennial tomatoes. For three years we failed to effect the grafting. In 1946 one of the flowers healed in, and the ovary

formed from it began growing and attained the dimensions of a medium-sized gooseberry. After that it stopped growing for a long time. At length, 2 1/2 months later, the ovary resumed growth, but now it was growing in an unusual way, suggestive of a gumboil. Somewhat later the fruit began growing on the other side, and successively assumed all sorts of shapes until its growth was complete. A similar variability of successive colours was also observed. At last the fruit was ripe. It was found to contain 643 seeds. Of this number 641 seeds were like eggplant seeds, and two differed entirely both from eggplant seeds and the seeds of the perennial tomato.

We planted these two seeds. One failed to germinate, but the other did. The plant grown from it was quite unusual in appearance, but it perished, and so I will not describe it. The remaining 641 seeds were perfectly alike. I thoroughly examined them under a magnifying glass, but try as I did, I was unable to discover the slightest difference between the seeds. I then decided to plant them, and at the same time planted some seeds of perennial tomatoes for checking purposes. Seedlings appeared, but as was the case with the seeds, not a single characteristic difference between them could be recorded.

A flea beetle attacked the seedlings, and began to destroy them. I was about to use pyrethrum in order to protect the plants from the flea when suddenly something attracted my attention. I noticed that some of the seedlings were not attacked by the fleas at all, and the seedlings of the perennial tomato were not attacked either. I have already published my observations (some of you have probably read my article in *Sotsialisticheskoye Zemledeliye* [*Socialist Agriculture*]) on "helpers without diplomas." Here is another case of the same kind.

I abstained from using the pyrethrum, and let them act freely. The result was that all plants but five were destroyed by the fleas. These five plants were not attacked at all though to outward appearance they were similar to the plants destroyed. It was obvious that in some respect at least these plants were similar in chemical composition to the perennial tomato plants.

A long interruption took place in my work on account of illness, but this year the grafting experiments were resumed. At the time I left for this session the eggplant fruit grafted on perennial tomato displayed considerable alterations. There was

no possibility of a sexual process having taken place, inasmuch as there had been no flowers on the tomato. The fruit of the control plants are pearlike, larger in the lower part, whereas the grafted fruit is sharp-pointed, similar to perennial tomato fruit.

In the preceding years we made similar experiments with the ovary of plums. We succeeded in effecting considerable changes in the colour of the fruits, from red to blue and from blue to red.

Last year flowers of a mandarin-orange hybrid taken from one and the same branch were grafted on orange and on mandarin. The fruits that grew from these flowers were found to have changed, but particularly characteristic were the changes observed in their seeds. From these seeds plants have been grown, which also show characteristic alterations.

All this evidence goes to prove that the only correct theory in biological science is that of Michurin, which allows the plant breeder to direct the process of development.

Of the other experiments made at the station I should like to mention the following:

Citrus hybrids require much time before they bear fruit. They do not come to fruit until the age of 7-15 years, or even later. Yet among grapefruit seedlings cases have been observed of fruiting in the first year of life. Formerly these phenomena were regarded as curiosities. But Michurinists consider them in a different light. From the seedlings that came to flower so early we took the pollen and transferred it to different citrus plants. This year early flowering was observed, not, however, in occasional instances, but on a mass scale. These seedlings formed ovaries, which attained the size of a cherry, but such fruits dropped afterwards, for reasons unknown to us. We are continuing our study of this problem. It will certainly be solved, and we hope to secure citrus hybrids that bear fruit at an early date.

That Michurin's theory on the important role of the environment in altering the nature of a plant organism is correct has also been shown by our station's experiments which are aimed at extending the cultivation of tea to the foothill areas of the Kuban, and also to the Caucasus. Tea was planted in all these more northern districts and the seeds obtained were

planted again until shrubs were obtained which withstood temperatures of more than 20° C below zero, without a cover of snow. Thus the cultivation of tea is beginning to spread northwards beyond the boundaries of the subtropics.

By using the methods indicated by Michurin, Soviet plant breeders have been able to produce the first home-grown varieties of citrus plants and other subtropical cultures, and are continuing along these lines. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician L. K. Greben.

Academician L. K. Greben. The question of the state of affairs in biological science, as raised by Academician T. D. Lysenko, is of enormous importance now in point of principle. We remember the discussions that took place in the years 1931, 1935 and 1938, when the representatives of formal genetics (Academician Koltsov, Academician Serebrovsky and others) laid claim to scientific leadership in the stock-raising industry of the U.S.S.R. Koltsov called on all zootechnicians to turn to geneticists for guidance, since, in his opinion, we were walking blindfold in science. He promised to rebuild the whole of zootechnical science on a new basis. There was even a saying at the time that "the sighted Gene will lead the sightless Phene." We zootechnicians were understood to be the sightless Phenes.

The late Academician M. F. Ivanov said in reply that in making an alliance with zootechny, geneticists should remember that zootechny was not coming empty-handed, that it had quite considerable practical achievements to its credit, and so geneticists must think the matter over again and again before they commit themselves to such great pledges.

I have recalled this incident in order to show that judging from the statements of various formal geneticists the reactionary school of Mendelist geneticists appears to have changed little in character, since they speak as presumptuously today as they did twenty years ago.

Academician Serebrovsky then proposed that the whole of zootechnical science of this country be placed under his direction. He wanted to establish a "gene committee" in Moscow, in which all sires—breed leaders—should be registered.

His leader theory, as we see now, and indeed as we could clearly see at that time, is an utter absurdity.

More than ten years have elapsed since the last discussion on problems of agrobiology. Agriculture in this country has undergone striking changes. The trend initiated in science by Michurin has become firmly established. Practical breeders and agricultural experts support it, too. Nevertheless we still continue to be faced with obstacles, which hinder our progress.

For us, pupils of Ivanov, his methods form the basis of our work. If we consider these methods in the light of Michurin genetics it is obvious to us that the views of the late Academician Ivanov were those of progressive science. His works written as far back as the years 1926-1928 contained conclusions which stated that not a single one of his experiments had corroborated the principles laid down by Mendel. At that time already Academician Ivanov met with opposition from the representatives of formal genetics. Thus Professor Vasin tried to disparage the work of Ivanov by proclaiming, without advancing any proof, that his experiments had been badly done and could not be relied upon. It goes without saying that similar attacks on our work may be expected from the same quarters in the future, and so we must be on the alert all the time, exposing at every step the onslaughts of these people who are impeding the development of agricultural science.

We, followers of Academician Ivanov, who work at Askania Nova, must at this conference declare our full agreement with Michurin genetics, the principles of which have been concisely and clearly elaborated and formulated by Academician Lysenko. We are thankful to him for having elaborated this theory, for it has enabled us to achieve further success in our practical work and has provided us with a theoretical basis for the further development of the methods employed by Academician Ivanov.

Particularly valuable to us, animal breeders, is Lysenko's thesis to the effect that the creation of good plant varieties and good animal breeds is impossible without good agrotechnical and zootechnical methods. On the basis of this thesis much can be achieved in animal breeding through environmental action.

Just what are these good zootechnical and agrotechnical methods? On this question objections are always being raised

by our opponents. This is quite understandable since they have no faith in the power of environmental conditions.

On the basis of practical experience, I consider that good zootechnical methods mean, first and foremost, favourable environmental conditions for the development of the animal organisms and next the proper choice and selection of animals, i. e., breeding according to Ivanov.

Why have I put these two points in the forefront? Because the results of practical work teach me that when we base our daily activities on Lysenko's theory and follow the methods of Ivanov we obtain better and better results from new highly productive Soviet breeds raised by Ivanov.

Yet according to the theory of formal genetics we should already be observing the steady extinction of the productivity indices of these animals, because of the impoverishment of the "gene store." But no such thing is observed. Here are some examples. The Askania breed of sheep suffered much privation during the war when evacuated to other regions, but in 1948 already, the productivity indices in the Askania Rambouillet flock were higher than ever in the history of this breed. In particular, individual animals of this flock are now showing record yields of pure wool such as are hardly equalled by any single sheep elsewhere in the world. Even in America there are no such sheep, though the American Rambouillet is so imposingly advertised that one might think they were working wonders there.

Why were we able in 1948 to obtain such high yields of wool? Why has one of the flock yielded more than 21 kg., as compared to 18 kg.—the maximum figure ever reached by a single sheep in the history of this breed? The sole explanation I give is that we are now equipped with the principles of Michurin genetics as developed by Lysenko, and also that in our work we follow the methods evolved by Academician Ivanov.

Outstanding results have also been achieved in respect to the yield of mutton by fine-wooled sheep. It has always been taken for granted that in fine-wooled sheep mutton yields are kept down by wool yields, and that by increasing the fleece we are bound to decrease the production of mutton. In actual fact, individual animals among the Askania Rambouillet flock show record figures of live weight. One of these animals weighs

157 kg. This figure, obtained in 1948, shows that the breed is growing not only as regards the fleece but also as regards live weight. In general such high live weights are unusual among fine-wooled sheep, only sheep grown for meat occasionally attaining so high a standard.

When we look at the Karavayevo herd of the Kostroma breed of cattle it is hard to tell whether these animals are grown for meat, or for meat and for milk, or for milk mainly and only secondarily for meat. They look as though they are grown for meat, but their milk records are also exceptionally high. And this is because man has raised these animals under appropriate environmental conditions.

I assert that our progress in sheep and hog raising is due to the application of Michurin genetics, as embodied in the methods employed by Ivanov. Now we are able to obtain from the animals whatever we want. I will give you another example, this time from the field of hog raising. Before the war the average litter for a herd of white Ukrainian steppe hogs was 9.6. At present the figure is in excess of 11 young at a farrow. The increase has been obtained by line breeding, which runs counter to the theory of Mendelism-Morganism. According to this theory we should observe extinction, as the line has been growing older, and the number of young per litter should show a decline. But there is not, and there could not be, any extinction, since the zootechnician who is able to apply all practical working methods and has mastered progressive theory will always achieve progressively favourable results, and not the contrary.

Within recent years a new breed of hogs, the speckled Ukrainian steppe hog, has been produced from the Ukrainian White breed at Askania Nova. Some scientists who think along formal lines refuse to recognize this breed as a new one. In their opinion it is genetically identical with the White breed. They say it is the Ukrainian steppe breed, but don't add that it is speckled, while they cannot call it a White one.

There are many similar discrepancies in zootechnical theory and practice. We who are guided by the principles of Michurin's progressive theory will have to overcome these discrepancies and wage a hard struggle against the opponents of this theory.

A serious defect in the field of zootechny, one requiring immediate attention, concerns errors committed in the raising of the Big White breed. Previously it used to be called the British Big White breed but latterly it is just called the Big White breed and is considered to be the very best for our country.

I recently inspected the herds on a number of sovkhozes, and as a zootechnician I must say, for all to hear, that the raising of the Big White breed of hogs here leaves much to be desired. When I examined the pedigree-books of the progenitors at the sovkhozes I found that hogs registered there by similar names were actually called by different names. Their "blood" (heredity) was similar, but they were known by different names. And they are used as different types of progenitors. Such a system of hog breeding at sovkhozes and kolkhozes cannot be considered correct. What should be done is to introduce the system proposed by Academician M. F. Ivanov, especially since as far back as the second session of our Academy, held in 1935, it was placed on record that Ivanov's method of linear stockbreeding should be extensively employed on the farms. We have to establish the constitutional type to which animals of different pedigrees belong, and what their productivity should be, for productivity varies in accordance with the different constitutional types of stock. Hence, every type requires the establishment of the proper conditions for high productivity.

I think the appropriate authorities will take this point into account, and so make it possible in the near future to eliminate the defects resulting from formalism.

A lack of clarity is also to be noted on the problem of the linear breeding of sheep. The method of linear breeding was elaborated and introduced for the first time here in the Soviet Union by Academician Mikhail Fyodorovich Ivanov. It fully proved its worth in connection with the breeding of the Askania Rambouillet, and so should be made the basic principle of the linear breeding of fine-wooled sheep in the U.S.S.R. All the principles of Michurin genetics must, of course, find their expression in this method. In addition, a whole series of principles of formal genetics now being operated in practical stock raising must also be overhauled. For example, there is the theory of lethal genes employed in connection with the breeding of grey karakuls. The production of grey karakul skins in

the Ukraine from Sokolki sheep and Karakul sheep is a very important matter since it is of great significance for the national economy.

But due to the operation of the theory of lethal genes, propounded by Academician A. S. Serebrovsky, all grey dams are covered by black Karakul rams, and this leads to the lambskins being a spoiled grey.

Then there is the theory of cryptorchism advanced by J. L. Glembotsky, a pupil of Academician Serebrovsky. This is also a harmful theory, for Glembotsky, anxious to prevent the existence of cryptorchism, recommends that Prècoce sheep be mated with merinos, that is to say, he proposes to turn a mutton-producing breed of sheep into one with a lower mutton yield, but, at any rate, with bigger horns. For the sake of eliminating cryptorchism each sheep is to lose dozens of kilograms of live weight. Apply this so-called theory to millions of sheep, and you will see what a tremendous amount of mutton is lost. Thanks for such theories!

We must not adopt a conciliatory attitude to them.

I shall not cite other examples. In general, we all of us have, as speedily as possible, to overcome the consequences of formal genetics which prevents the progress of our stock-raising industry, the advance of the progressive agrobiological theories of Michurin, as developed by T. D. Lysenko.

We may be confident that it will not be long before Michurin's teachings win general recognition. (*Applause.*)

Academician P. P. Lobanov. I call upon Comrade V. S. Dmitriyev, Chief of the Agricultural Planning Administration of the State Planning Commission of the U.S.S.R.

V. S. Dmitriyev. Comrades! The problem being discussed at this session is, as was quite correctly emphasized in his address by Academician T. D. Lysenko, of significance both for biology and agricultural science. There is, however, a certain defect in the way the problem has been treated here. It consists, to my mind, in the fact that not sufficient attention is being devoted by speakers to questions associated with other sciences directly related to biology and to the problem of improving the crop yield in our country.

I think this is due to the fact that some of our scientists, at work in various fields, such as soil science, irrigated farming, etc., believe that the struggle now being waged against backward, reactionary theories, concerns only biology, or even only genetics, while as for the rest of the sciences the old state of affairs will remain unchanged. That, in my opinion, must not be permitted.

The fact is that in the postwar period we have been confronted by great difficulties in our efforts to restore agriculture. And in spite of these enormous difficulties—the aftermath of the severe losses suffered by our farming during the war and the severe drought of 1946—considerable achievements have been made. We have made tremendous progress in restoring agriculture. This is conclusive evidence of the fact that our socialist system of farming as established by Lenin and Stalin, is the most advanced, progressive system ever known in the history of farming. And among the conditions necessary for the success of the kolkhoz system, one of the most important is precisely the fact that our kolkhozes and sovkhozes assimilate all the latest achievements of agricultural science.

As Comrade Stalin has pointed out, the problem to be solved by the five-year plans of the next period is the creation in our country of the abundance of articles of consumption, necessary for the transition from Socialism to Communism. A particularly heavy responsibility falls upon those occupied in agricultural science in connection with the solution of this great task.

This requires that the most valuable achievements of science be applied to practical farming. It requires, in addition, the further advancement of agricultural science, of all its aspects, since they are of tremendous importance in raising the crop yield and improving cattle breeding at our kolkhozes and sovkhozes. Under these circumstances, then, we cannot, of course, remain indifferent to the appearance of a whole number of scientific works which not only fail to assist, but actually disarm our practical agriculturists. You are already aware of the fact that in Academician Schmalhausen's book, *Factors of Evolution*, published in 1946, i.e., after the war, when we were already faced with the problems I have mentioned above, one of the fundamental ideas—of considerable practical signif-

icance—developed by the author is that of the steady extinction or slowing down of the process of breed or variety formation both in animal and plant husbandry. This, he asserts, is due to the exhaustion of the store of hereditary substance, a store which is supposed to have been deposited at some time or other by somebody in the so-called genofund.

But this work is not the only one of its kind, and has already been estimated at its proper worth here. In 1947 there appeared a book by Professor Rodé, entitled *The Process of Soil Formation and the Evolution of Soils*. In addition to the factors of soil formation as established by Dokuchayev and Williams, the author introduces a number of new ones, including gravitation and the influence of sunspots. He has a special thesis about the influence of sunspots upon the process of soil formation.

This thesis has, of course, nothing in common with science; but that did not prevent the publication of the work in which this essentially obscurantist theory was developed.

What is more, Professor Rodé's work is a direct echo of Academician Schmalhausen's book on factors of evolution. Professor Rodé supplements Schmalhausen's idea of the decline of breed and variety formation with that of the steady decline of the process of soil formation.

"Two main periods can be distinguished," writes Professor Rodé regarding the process of soil formation, "one, proceeding at a relatively high rate, is the period of soil formation, and the other, during which the process is considerably slowed down is . . . a period of its slow evolution"; and further: "the process of soil formation proceeds at a steadily decreasing rate."[1] According to Rodé, it appears that at the very beginning of soil evolution, when milleniums were necessary for the first scanty vegetation to appear upon weathering products of rocks, soil formation went on at a more rapid rate than now, when man has become the decisive factor of soil formation, and when, under the conditions of the socialist system, man has at his disposal truly inexhaustible means for increasing soil fertility and raising the crop yield.

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[1] А. А. Роде, *Почвообразовательный процесс и эволюция почв*, Москва 1947 г., стр. 135.

Who, then, will place faith in this soothsayer who has chosen to intimidate us at a time when we are passing to Communism?! We thus see that according to Schmalhausen the processes of breed and variety formation are slowing down, while according to Rodé the same is also true of the process of soil formation. But that is not all.

In 1947 a big work of two volumes appeared, written by Professor Kovda, under the title *Origin and Regime of Saline Soils*. In his work Professor Kovda virtually disseminates and upholds the "theory" of the unavoidable salinization of soil, as advanced by American irrigators.

In his book we read: "As a result, irrespective of whether the watering of an irrigated area is accompanied by rigorous limitation of the water supply, so that the water-retaining capacity of the soil is not exceeded, or whether the amount of water supplied exceeds the water-retaining capacity of the soil, in either case, and in the latter case in particular, the accumulation of salts will proceed at an especially high rate under the influence of the salts carried in with the irrigation water."[1]

"In certain landscapes the economic activities of man will be accompanied by processes of soil salinization, altogether irrespective of these activities, in particular of irrigation."[2]

What follows from this? In the most important sciences—in those concerning the development of the vegetable and animal kingdoms, in the domain of soil formation and in that of irrigation farming—theories are developed, according to which the future holds nothing good in store for us. Objectively, all such theories lead to a loss of faith in the possibility of Communism being victorious in our country. And whether the authors want it or not, objectively they play into the hands of those who are opposed to Communism, i.e., to all that is advanced and progressive.

All the works mentioned have been produced within the walls of the highest scientific institution in our country, namely, the Academy of Sciences of the U.S.S.R. This shows that obviously all is not well, as far as science is concerned,

[1] В. А. Ковда, *Происхождение и режим засоленных почв*, том I, Москва 1947, стр. 45.

[2] *Ibid.*, Vol. II, p. 280.

at a number of the Academy's institutes. I think therefore that the Lenin Academy of Agricultural Sciences of the U.S.S.R., which has taken the initiative in conducting the struggle against reactionary theories in the field of biology and agronomy, will do the right thing if it asks the Academy of Sciences of the U.S.S.R. to overhaul its institutes and to freshen up the musty and reactionary atmosphere that has accumulated in some of them.

I am sure that it would not be a bad thing for such initiative to be displayed by the Lenin Academy of Agricultural Sciences of the U.S.S.R.

I think that in general one of the most important conditions for the further advance of science is that an end be put once and for all to the "lone-furrow farming" in science that still goes on under the name of "scientific schools." All branches of science must be developed on a single basis, the only scientific one, namely, that of dialectical materialism, on the basis of the teachings of Marx-Engels-Lenin-Stalin; no attempts must be made to preserve and reconcile conflicting tendencies in science. There can be no reconciliation between materialism and idealism, dialectics and metaphysics, Michurin's doctrine and Mendelism-Morganism.

Fortunately, Russian Soviet agronomical science has supplied us in our country with a harmonious general theory of agronomy, a theory of the continuous increase of yields; this theory has not only facilitated the successes of our Stakhanovites, but has provided the kolkhozes and sovkhozes with the basis for further increasing the yields of farm crops, and for creating an abundance of articles of consumption in our country.

This theory is far superior to the West-European doctrine of crop succession, and our Soviet science is rightly proud of it. The theory is called the Dokuchayev-Kostychev-Williams complex, or the travopolye system of agriculture.

But while Dokuchayev, Kostychev and Williams mainly developed methods of influencing the soil, of obtaining highly fertile soils, Timiryazev and Michurin were mainly concerned with developing methods of influencing the plants which, as Timiryazev put it, constitute the main object of the farmer's activities. This theory too should be further developed.

Any agricultural theory, if not elaborated will not only fail

to make progress as a theory but will be prevented from being applied to practical farming.

The most progressive feature of the Darwinian doctrine is just the fact that Darwinism is a theory of the development of the vegetable and animal kingdom, and, being a theory of development, does not tolerate stagnation. Darwin, the founder of the theory himself, emphasized on more than one occasion his desire to improve the effectiveness of his theory, to increase its practical application and value. Darwin used to declare that any new variety raised by man would be a more important and interesting subject for study than one more species added to the infinitude of already recorded species.

The doctrine of the travopolye system of farming, which is a system of measures designed to increase soil fertility and to obtain higher crop yields, is being continuously improved and perfected. In many cases it could not possibly be applied without further development. But we can only find this development in research work of the Michurin trend, and primarily in the work carried on by Academician Lysenko. I shall demonstrate this by a number of facts that are known to you all and especially to those who are providing for the practical development of grass sowing in our country.

As is well known, the cultivation of grasses has made extremely slow progress, especially in the chernozem steppe areas. I shall not deal with the general economic factors which handicapped the introduction of all that was progressive, including the cultivation of grasses under the landlord-capitalist regime, but I must say that one of the factors responsible for the inadequate development of grass cultivation was precisely the faulty technique applied to grass cultivation, and particularly to seed growing of perennial grasses within the chernozem steppe areas of our country, which constitute one of the most important bases of agriculture in our country.

This was frequently stressed in his day by the well-known Russian agronomist, Professor P. A. Kostychev.

More than fifty years ago, Professor P. A. Kostychev made the following statement in his public lectures entitled "On the Struggle Against Droughts in the Black-Earth Belt by Cultivating the Fields and Accumulating Snow on Them": "We have sustained heavy losses because of our adoption of

West-European standards of soil cultivation. Our failures in grass sowing have been equally due, in my opinion, to the fact that the sowing of grasses has been practised almost exclusively according to the methods recommended in Western Europe, to whose climate and soils they are well adapted, while they seem hardly to fit our conditions. We used to sow forage crops along with a cover crop such as oats, wheat, etc. Although by the time the cover crop ripens the grass crops show an inconsiderable growth, and only begin to develop after the cover crop has been harvested, nevertheless, a field sown with grass has to provide nourishment for two cultures, whereas the soil is mostly so dry that only one of them can attain full growth . . . the soil is able to produce either corn or grass, but cannot produce both at one and the same time."[1]

This thesis of Kostychev was not only not developed but was even forgotten, and only now, after the outstanding successes achieved in the summer sowing of alfalfa on bare fallow land practised in the South according to the method advanced by Academician Lysenko, are these principles apparently being revived.

I cannot help citing the data reported by the Director of the Dokuchayev Institute of Agriculture of the Central Black-Earth Belt, Comrade Krylov. Sowing alfalfa on bare fallow last year, they collected 5.8 c. of alfalfa seed per ha., instead of the 1.5 c. collected after the usual method of sowing. It is perfectly obvious that the problem of alfalfa sowing in the South has now been solved in science; it only depends now on those engaged in practical agriculture how soon and to what extent this procedure is applied, and at what rate grass sowing is adopted in the steppe regions as one of the most important links of the travopolye system of farming.

Here is another example, likewise associated with the introduction and application of the travopolye system.

The outstanding importance of the afforestation of our steppes is now generally acknowledged. In spite, however, of the obvious necessity of the afforestation of watersheds,

[1] П. А. Костычев, *О борьбе с засухами в черноземной области посредством обработки полей и накопления на них снега*, Москва 1914 г., стр. 81-82.

ravines and other kinds of unworkable land, as well as of creating a system of shelter belts, very little has been done in this direction.

The questions as to the varietal composition of the forests to be planted have not received due attention, problems concerning the technique of silviculture in the steppes have been neglected and muddled; and even the question as to the width of the protective forest belts, which never gave rise to any doubt for fifty years, has now become obscure, as a result of hasty, I would say superficial, investigations carried on by the All-Union Institute for Reclamation and Afforestation.

Voice from the hall. That's right!

V. S. Dmitriyev. Under these circumstances, steppe afforestation was endangered, but here too we are helped by Michurin's teachings and Academician Lysenko's research on steppe afforestation.

These investigations have given a new aspect to every theoretical and practical problem facing us.

The history of steppe afforestation shows that one of the first attempts to afforest the steppes was made by Peter the Great, who as far back as in 1696 ordered acorns to be sown near Taganrog. This is the origin of the forest massif known in Taganrog Region as the "Dubki" boundary.

Inasmuch as this fact has been cited in several textbooks, one may suppose that the method of forest growing by seeding has long been known, as also the fact that steppe afforestation should be started by sowing oak rather than locust trees. Yet neither forest growers nor agronomists seemed to attach any importance to the facts they cited.

In 1843 Graff, who for some reason or other is considered to be the first Russian silviculturist, put down the famous Veliky Anadol forest. The method of artificial reproduction employed was the so-called nursery method. Planting was done into pits 21 in. wide and deep, with one tree, 5-6 years old, for every 50 sq. ft.

Planting was preceded by fourfold ploughing in the course of two years. After planting, the forest required management for a period of 10-11 years until the crowns were closed. Within this period, some 32-36 treatments were made, including slash and weed disposal.

In spite of the fact that the work was carried out under serfdom, when labour involved practically no expense, each dessiatin of forest plantings cost 700 gold rubles.

It is quite obvious that this method was not the best one, and a hard struggle ensued for the improvement of methods of forest planting. It should be pointed out that quite an outstanding role was played in this regard by the silviculturists Tikhanov and Tursky, and by the agronomist P. A. Kostychev, who was one of the first in our country to raise the issue of employing truly scientific methods in steppe afforestation.

It was established by Kostychev that the only obstacle to successful forest growing in the steppe areas is the competition of wild herbaceous plants. "In general all observations made in the forests [steppe forests—*V. D.*] described above, lead us to the conclusion that the competition of herbaceous plants is the only obstacle to the afforestation of the steppes."[1] Kostychev pointed out that as forest planting in the steppes was investigated, the technique of reproduction was increasingly improved, so that "now use is made of the simplest methods of reproduction, consisting solely in the removal of wild herbaceous plants which compete with the newly planted trees during the first years of their life."[2] Citing M. K. Tursky, P. A. Kostychev pointed out that in advanced forest districts the procedure of reproduction consisted in ploughing and harrowing, planting, dressing of autumn plantations in spring, and in weed disposal during a period of only 3 years, instead of the 10-11 years stipulated by Graff's method, with a total of 10 procedures instead of 32-36. "By the fourth year," wrote P. A. Kostychev, "the young trees will have closed their crowns, and then they will no longer be endangered by the competition of wild-growing plants; the existence of the forest on the given plot may be considered safe for ever."[3]

One would have thought that this method ought to be still further simplified, for since the main obstacle to forest re-

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[1] П. А. Костычев, *Почвы черноземной области России. Их происхождение, состав и свойства,* 1937 г., стр. 126.

[2] *Ibid.*, p. 123.

[3] *Ibid.*, p. 125.

production is wild vegetation, as was also admitted by Academician Vysotsky, this vegetation could be removed more efficiently and at a lower cost. This can be secured by creating such conditions as would make the trees close their crowns at an earlier date. Actually, something different occurred.

So-called science recommends that we plant narrow ranges separated by intervals that are too wide, the plants themselves to be 0.5 or 0.75 m. apart.

No wonder, then, that in many regions more than 50% of 6-10-year-old trees planted by kolkhozes in order to form protection belts have still failed to close their crowns, and that their management still requires enormous outlays.

Such a method of steppe afforestation was due to the fact that some so-called "Darwinists" asserted that the chief enemy of each young tree in the steppe is its neighbour-tree, rather than the wild-growing grasses around it. Hence they considered it necessary to plant the trees as far apart as possible, and to make the middles as wide as possible. As a result, the expenditure was enormous, while the growth of trees in the steppes was poor.

The only correct solution of the problem is the one proposed by Academician Lysenko, and the sooner we put his suggestions into practice, the sooner and more successfully shall we solve the very imposing problem of the afforestation of the steppe belts of our country.

To introduce and establish the travopolye method of crop rotations, it is necessary to effect considerable changes in the arrangement of the cultivated areas, with regard to the ratio of the various crops. Learned agronomists and economists as a rule emphasize the necessity of doing so, but their suggestions are not carried to a definite practical solution of the problem, and poor results are obtained.

Because of the random and extremely antagonistic specialization of farming under capitalism, in many districts highly unfavourable ratios of cultures were formed: as much as 90% of the total cultivated area in these districts were under cereal crops. The first propagandists of crop succession in our country were forced to stress the necessity of introducing the cultivation of root crops and tuber crops, in par-

ticular the potato, in all regions including the South. The methods used for potato growing in the South were a mechanical repetition of those employed in the northern regions of the country, or were brought in by the landlords from abroad. The results obtained proved, of course, anything but favourable: the planting material degenerated, and this poorly transportable culture had to be supported at the cost of planting material obtained from the country's northern regions. These conditions continued until Academician Lysenko suggested the summer planting of potatoes in the South. In the southern regions we can and must have as many potatoes as we need, and I am not sure where we shall get better crops, near Moscow, from Comrade Arnautov, or near Odessa, from Academician Olshansky.

One of the most important problems of lea crop rotations in the southern provinces of the Ukraine is associated with a considerable increase in the areas under technical crops, especially under that highly important crop, cotton. Michurin's teachings have proved helpful in this case, too; a variety of cotton has been created for the new cotton-growing regions, and the topping of the cotton plant suggested—a process of enormous importance to all cotton-growing areas and now being applied on a large scale.

One can hardly consider it a blessing that a number of regions grow exclusively winter wheat, or that only spring wheat is grown in the Volga area or in Siberia.

The principles of lea crop rotation require that the appropriate correction be introduced in this regard.

The fact that farming is conducted in this country according to a general plan, opens up boundless possibilities for the rational distribution of agricultural crops and for combining them in a way optimal for a given belt. But this is no simple matter.

At one time the best varieties of spring wheat disappeared in the southern regions, and until recently every variety of winter wheat used to perish in Siberia. This state of affairs in science has now been changed.

I shall not dwell here upon the work done by the Odessa Institute of Selection and Genetics and by other institutes which are successful in breeding good varieties of spring

wheat for the Ukraine and for North Caucasus. Reference to them has already been made here. I should like to stress the point that much more has been done for Siberia than the production of a single variety. A tremendous discovery has been made in this sphere by Lysenko. He has established the point that every variety of winter wheat can survive the Siberian winter, provided rational methods of agriculture are applied. I refer to the sowing of winter wheat on stubble land.

But what resistance this came up against! Excuse me if I put it strongly, but it was like that of wild animals. The enemies of progressive tendencies in science, in defending their obsolete views, resort to wrong, impermissible methods, a point that should be placed on record and condemned. Is it worthy of a scientist to behave as Professor Rapoport did yesterday?

Voice from the hall. It was ruffianism!

V. S. Dmitriyev. It should not be left at that.

Voice from the hall. Quite right!

V. S. Dmitriyev. Such behaviour should be severely condemned.

I should like to quote another example illustrating a method of polemics hardly in keeping with a scientific dispute: I allude to the discussion that took place at the All-Union Association of Soil Scientists. During a meeting of this body, when Rodé's book, which contains a number of gross errors, was under discussion, Professor Bobko used absolutely impermissible language.

In the same speech Professor Bobko condemned wholesale a number of highly valuable agricultural procedures suggested by Academician Lysenko, including the bunch planting of kok-saghyz; in his view the only result of this latter method is that "the bulk of the combined fertilizer-and-seed drills have been taken away from corn-crop farms and given to those cultivating kok-saghyz." He also found fault with the cultivation of winter wheat in the Siberian steppes, etc.

The underestimation, and even outright ignoring of the services of the coryphaei of Soviet agricultural science, Michurin and Williams, as well as the pooh-poohing of the work of young Soviet scientists, is one of the improper methods

employed by those who advocate backward, reactionary doctrines.

A stop must be put to this.

The most "formidable" objection to the agricultural technique suggested by Academician Lysenko for the cultivation of winter wheat has been that his methods run counter to those applied for centuries past.

There is more than one agronomist whose idea of progressive agriculture is still based upon Cato's winged phrase: "plough, plough again, and fertilize." And from this angle, such agronomists cannot understand Lysenko's proposals. Lysenko does not base himself on what was said by Cato, but on what a plant requires for its successful development.

And if all our agronomists were to approach matters in that way we would find correct solutions for very many problems concerned with the evaluation of various procedures.

In working out agricultural procedures, one should always bear in mind—and this is one of the traits characteristic of Academician Lysenko—that each agricultural procedure must serve to secure an increase in the yield of a given crop per hectare, with the minimum outlay possible.

This economic aspect of the problem, the calculation of what a new agricultural procedure will cost, has been badly neglected by all our agricultural research institutes, including the Academy of Agricultural Sciences. An end should be put to that state of affairs.

I cannot help emphasizing here the exceptional importance of the work done by Academician Lysenko as regards branched wheat, particularly his work in introducing this crop in the vicinity of Moscow. The suburban regions in general, and especially such an important zone as that surrounding Moscow, the capital of our great Motherland, need an enormous supply of agricultural products, including cereals. However, we cannot set aside large areas around Moscow for the cultivation of cereal crops, hence we must in this region obtain a maximum of grain from a small area. This problem can be solved by introducing the cultivation of branched wheat along the lines demonstrated by Academician Lysenko at Gorki-Leninskiye.

I should like to close my remarks by advancing the following highly important conclusions:

1. We are in possession of a doctrine, involving a system of agricultural measures intended to provide for a continuous increase in yields and an abundance of agricultural products in our country. This system has been worked out by those outstanding representatives of Russian agricultural science—Dokuchayev, Kostychev, Timiryazev, Williams. This doctrine is not only organically linked up with the Michurin doctrine, but has been raised by the latter to a new, higher level of development. Moreover, by developing in every way the scientific foundations of the travopolye system of farming, the doctrine of Michurin and the works of Lysenko have made this theory more effective and more widely accessible.

2. The achievements of Soviet agricultural science, including those of Soviet agrobiology, cannot be better characterized than was done by the late Academician V. R. Williams in one of his last articles.

He wrote the following: "It can be asserted without exaggeration that we are gradually becoming the real 'masters of nature,' because our advanced agricultural science has to a great degree learnt objectively to understand the laws of nature, and utilizes them in the interests of the present generation of our Socialist Motherland, and of generations to come.

"This has become possible in our country alone, where the triumphant theory of Marx-Engels-Lenin-Stalin holds undivided sway. The vital forces of this theory have rejuvenated the old decaying agronomy."

3. I am sure I express the desire of everyone present at this session in wishing the Academy of Agricultural Sciences, and its new members in particular, that they further advance Soviet agricultural science, and in such a way as to secure the abundance of products that we need in order to pass from Socialism to Communism. Agricultural science must be developed in the way we are bidden to do by the great coryphaeus of science, our teacher and leader, Comrade Stalin. (*Applause.*)

Academician P. P. Lobanov. I now call upon Professor K. Y. Kostryukova of the Kiev Medical Institute.

Professor K. Y. Kostryukova. In his letter to the students of the school at Capri, V. I. Lenin wrote: "The most important thing in every school is the ideological and political trend of the lectures."[1]

This principle is perfectly clear to us, university teachers, and to teachers in general. We feel a deep responsibility for the education of our young people, the future builders of Communism. But the education we give derives primarily from the ideas contained in our lectures. One can therefore understand what enormous damage can be done to the work of socialist construction by a lecture whose ideological level is not high enough, to say nothing of a lecture that is simply reactionary.

The facts related here yesterday by the head of the Department of Philosophy of the Moscow University were therefore dreadful, indeed. For a number of years, young biologists of the Moscow University have been educated in the spirit of reactionary theory.

It should be pointed out, however, that the harm done by the biologists of the Moscow University extend far beyond its walls. The biologists of the Moscow University constitute a large body. It is from their ranks that members of the editorial boards of biological periodicals were selected; and they too provided the reviewers of articles on biological problems submitted for publication in these journals. No wonder, then, that over a period of several years such biological periodicals as the *Journal of General Biology, Bulletin of the Academy of Sciences of the U.S.S.R.* (biological series), the *Doklady of the Academy of Sciences of the U.S.S.R.* (in so far as biological problems were concerned), never published a single article of the Michurin trend. Thus quite a peculiar assortment of articles resulted, and consequently these periodicals served to spread Morgan's doctrine far and wide over the immense territory of our country.

The Ministry of Higher Education of the U.S.S.R. is

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[1] В. И. Ленин, *Сочинения*, том XIV, изд. 3-е, стр. 118.

situated in Moscow; this Ministry sanctions syllabuses and textbooks meant for the whole of our country. Who are invited to give their views on these syllabuses and textbooks? Again, these same biology experts who live in Moscow. The influence of Moscow's biologists is to be felt everywhere.

I happened to experience this influence personally during the years of the war. In 1942 the Kiev Medical Institute, where I am the head of the Biological Department, received a syllabus, sanctioned both by the Committee on Higher Education and the Ministry of Public Health of the U.S.S.R. The ideological content of this syllabus was of so low a level that I immediately wrote a memorandum containing my views to the head of the Institute. The latter approved of it and sent it to the Committee on Higher Education. Moreover, a letter was sent to the *Meditsinsky Rabotnik* (*Medical Worker*).

It should be stated that this syllabus propagated bourgeois genetics. To form some idea of it, suffice it to say that the name of the great biologist and transformer of nature—Michurin—was never once mentioned in this syllabus, which was meant to cover the field of general biology.

Allow me to quote the introductory part of my letter.

"The heroic struggle of the Soviet Union against a brutal enemy possessing the strongest and best-equipped army in the world, and the brilliant successes achieved in this struggle, naturally evoke a growing feeling of legitimate pride and patriotism among the peoples of the U.S.S.R. At a time like this, the achievements of our people become particularly dear to us, and especially do we hold dear our patriotic Soviet scientists, creators of Soviet biology. There are a number of progressive scientists, active in the field of Soviet biology; our people know them and hold them in high esteem. Their works serve as material for the education of the young generation, who become acquainted with them while still at school.

"But there is one field of work that is not disturbed either by emotion or joy, by fervour or patriotic enthusiasm. It is a field where everything is calm and peaceful, where thought lazily slumbers on, and where science has come to a halt at the point it reached twenty-five years ago. I refer to the syllabus on biology for medical and stomatological institutes, issued by the Committee on Higher Education in 1942."

An answer came very soon. I was invited to draw up a draft syllabus on general biology for medical institutes. I did so, and dispatched it in due time.

However, nothing has been heard of it since. The years 1943, 1944, 1945 passed by, the war came to an end. And there was still no word about the syllabus. What did that mean? It meant that the old syllabus continued to be in use.

Finally—in 1946—a new one appeared (it had been published in 1945, but we did not receive it until 1946). But this syllabus was worse than the old one. If my first memorandum ended with the words: "the syllabus on biology needs urgent revision," my second memorandum, which I sent in without delay, contained the words: "This syllabus may do much harm, it should be cancelled immediately." The syllabus was compiled in such a way that I would have felt ashamed had it fallen into the hands of a student who had studied under me for even three months.

The year 1946 passed by, and so did the year 1947. In the spring of 1948 we received a note from the Ministry of Higher Education. This note, on half a sheet of paper, contained the instruction to insert on page so-and-so the name of Michurin, and add the name of Schmalhausen after such and such words on page so-and-so, etc. I sent in a protest, but so far no answer has come.

I understand now what is the matter: the syllabus was handled by individuals who were concerned to propagate the views contained in the syllabus.

I want to say that all this—the syllabuses, the teaching that is done according to them, the textbooks that are written in accordance with them—does enormous damage, to a degree you can hardly imagine. This has repeatedly been pointed out from this rostrum. The fact is that our young people eagerly absorb the knowledge made available to them in the universities. Occasionally some of the ideas become so deeply rooted that they are remembered for a lifetime. That is why it is absolutely necessary to take immediate steps to prevent the occurrence of such facts as those that have been described here.

Yesterday we listened to Comrade Rapoport's speech from this rostrum. His speech was that of a real, convinced follower of Morgan. He has been captivated by this hostile theory. Com-

rade Rapoport defended Morgan's theory to such effect that it seemed at first that everything was right with it. The gene was presented in new, fashionable attire, wearing biochemical dress. We were now being told of the genohormone.

But you should be honest, Comrade Rapoport! You should say that the new hypothesis cited by you was a mere hypothesis with nothing to support it, whereas you presented it as an undoubted truth. It should be said that the presentation of such unproven hypotheses as undoubted truths is typical of Morganism. It is typical of the founder of the theory, Morgan himself. Morgan did not recognize the word "hypothesis" at all; even the word "theory" was beneath his dignity. All the notions he invented bore the names of laws. In any textbook on genetics you may find the law of crossing over, the law of the linear order of the genes, etc.

I would say that this method of presenting hypotheses without producing evidence in support of them is a manifestation of the inordinate pride of the adherents of Morgan's theory. We have already heard that their pride is additionally displayed in their assumption that they are the only genuine scientists. They do not even condescend to criticize other theories.

What is the basis for the pride of the adherents of Morgan's theories? Their doctrine, as they themselves define it, is that of the gene. The gene is the central concept of Morganian genetics. What is the gene as defined by the Morganists? The gene is a particle of substance. So we were told yesterday by Comrade Rapoport, who laid special stress on this point. But what sort of substance is it? It is a peculiar substance, which serves as the bearer of heredity. We thus see that, according to the doctrine of the Morganists, there is a special substance that serves as the bearer of heredity, whereas the rest of living matter has nothing to do with heredity. Just imagine what is being said! Heredity, which is an attribute of the living body, is separated from it, set against it.

In an article published in 1936 in our periodical, *Priroda*, Muller states this point very clearly. He says that the cell contains a nucleus and protoplasm, but not all the living matter in the cell serves as a bearer of heredity. Only an insignificant part of the cell—the chromosomes—are endowed with this property. True, they now say that genes are also to be

found in the plasm of the cell. But that does not alter matters essentially. The plasmogene and the chromosomal gene are both made of the same peculiar hereditary substance. Such peculiar, concocted substances or forces were known in many other sciences at the initial period of their development. They were intended to provide an explanation for phenomena which could not be explained at a given stage of development of this science. Thus, for example, in physics the caloric was invented to account for thermal phenomena, and in chemistry, the phlogiston served to explain the phenomenon of combustion. In biology recourse was had to a vital force, in order to account for obscure vital processes.

The properties of substances are thus separated from the body, opposed to the latter as some independent entity. The same picture is to be observed in genetics: heredity—a property inherent in each living being—is isolated from it and counterposed to it, as some kernel, hereditary substance. It is this hypothetical substance that is studied by geneticists. They even connect it with a material substratum, the chromosomes, and see therein the confirmation of its existence. This, however, does not deprive this substance of the qualities that distinguish other invented substances. The hereditary substance that is set against living matter exists no more than do either the caloric or the phlogiston.

This contraposition brings to light the downright dualism typical of every idealistic, vitalistic interpretaton of life. So that, it turns out, is what the Morganists are so proud of! The gene is an utter fiction, however much you, Comrade Rapoport, may assert that it is a particle of matter. The electronic microscope will not save you. You may see under an electronic microscope whatever minute particles you like, but they will still be nothing other than particles of a chromosome, and you will never see a gene, for there is no such thing, no more than there is a vital force.

It thus turns out that the science of the gene is in the prescientific stage of its development. The science of the gene is a false theory which impedes the development of science. Engels once splendidly described the role of such theories in the development of other sciences. Unfortunately, I have not at hand his book *The Dialectics of Nature,* and I cannot quote it. Engels

says that Carnot Sadi was on the point of reaching the very essence of the problem, but he was prevented from solving it by the fallacious theory of the phlogiston.

During the first discussion, that took place as early as 1936, Comrade Lysenko pointed out that the Morganist-Mendelists had become confused in their understanding of development. And it is this that constitutes the essential difference between the Michurin trend and that of the Morganists. The theory of Michurin is one of development. Michurin's immense achievement in science consists in that he was the first to give a consistent picture of development, as it takes place within the life of the individual. He showed how, during the life of the individual, changes arise and are formed, that subsequently become the basis for the formation of a new species, the basis for phylogenetic development.

Michurin put his theory into practice. This is why we say that Michurin raised Darwinism to a higher level of development.

As to the Morganist-Mendelists, they abhor the theory of development. The Morganists now keep silent about the fact that the founders of their theory themselves held the viewpoint of the invariability of the gene (their basic concept), that they waged a long struggle in which they upheld this invariability. We still remember the time when, during our first discussion, there were those who advocated the invariability of the gene. They would be embarrassed to say that just now, and in his speech Comrade Rapoport pointed out that the variability of the gene is acknowledged by Morganists. But variability may be of different kinds: you can kill an organism with a stick, the organism will suffer a change, but there will be no development in that case. The action of mutagenic substances is the same as hitting an organism with a stick: this is why their effect is the same as that of the blows inflicted by a stick. Morganists are unable to explain how hereditary changes arise. They have created a deep gulf between modifications and mutations. Their view is as follows: Mutation is not a historical category. It appears at once in a ready-made form. It undergoes no process of formation, its quality is not affected by environment. Moreover, it is not linked in any way to preceding mutations. It is clear, therefore, that being theoretically convinced of this

character of variation, they find it impossible to try to influence it. How can one influence a variation which is not connected with anything, and arises all of a sudden, ready made?

This is why the theory of the Morganists does not equip them for practical work, but on the contrary, makes them helpless.

Comrade Rapoport did not give a direct answer to the question as to whether he admits the inheritance of acquired characters. Had he been frank about it, he would have said outright that he denied it, and at the same time denied all the practical achievements of Michurin genetics, all its theoretical principles.

But Comrade Rapoport could not be as frank as that. I must, however, say that of late frankness is not a quality typical of the Morganists. They are frank abroad. One frank avowal, it is true, made its way into our press, but that, apparently, was at a time when Morganist tendencies seemed to be making great headway. I am referring to the article by M. M. Zavadovsky, entitled "Thomas Hunt Morgan," in which he quite openly took a stand in favour of Weismannism.

Neither Comrade Rapoport nor any of the other Mendelists have the courage to propound their theoretical views with such frankness. The history of science contains other such cases. Reactionary theories are very often camouflaged, and their reactionary essence, their bonds with other reactionary theories are hidden.

K. A. Timiryazev, who was an irreconcilable enemy of such idealistic, vitalistic theories, hit the nail on the head when he nicknamed them "kinless." Our Morganists are nothing but kinless Weismannists! (*Applause.*)

Allow me to dwell very briefly upon the quality of the evidence sometimes adduced by Morganists in support of their theory. Everybody knows of the tremendous significance attached by Morganists to cytological evidence. There is one item of cytological evidence, borrowed from plant embryology, to which particular importance is attached. This evidence is the structure of male gametes in angiosperms.

In 1910 our eminent scientist, S. G. Navashin, described male gametes with a structure of naked nuclei, in the classical object of cytological investigation, namely, *Lilium martagon*.

In the textbook on genetics by Grishko and Delone the section on cytology contains the statement that this fact is of enormous theoretical importance, since it testifies to the preponderant role played by the nucleus in phenomena of heredity.

The textbook was published in 1939, while the data cited were those of the year 1910.

However, as is well known, Navashin was not only a good observer, but an outstanding expert in microscopical technique. The art of producing preparations was carried by him to a high degree of perfection. He continued to elaborate this technique throughout his life. He used to say that vital observations would be highly desirable, and bitterly complained of his failure in this direction.

He trained his pupils in the same spirit. It is a well-known fact that the greater part of Navashin's creative work was done in Kiev, at the very time when he was engaged on the research that brought him his well-earned fame. It was there that he founded his own school of Kiev embryologists. Their training was based on Navashin's principles, and aimed at improving their technique of producing preparations, of microscopy, of drawing. Hence there is nothing fortuitous about the fact that before long, the school that had been founded and trained by Navashin began to produce data demonstrating that angiosperms contain male gametes, appearing as well-shaped cells. The first to observe them was Navashin's closest pupil, V. V. Finn, who was followed by several other investigators. Somewhat later, Navashin's pupils succeeded in giving effect to an idea he had bequeathed to them: they elaborated a method of vital observation under a high-power microscope. Men like M. V. Chernoyarov, who had studied under Navashin himself, and other scientists who had not known him personally but had been trained in his school—succeeded in demonstrating upon material *in vivo,* that no naked cell nuclei ever occurred in a living and growing pollen tube in which streaming of cytoplasm and movements of cells were observable. The sperms invariably represent intact, well-shaped cells, showing no tendency to leave the nucleus naked.

Only part of these investigations have been published, appearing in the periodical *Yarovizatsia* (*Vernalization*) just be-

fore the war. The photomicrographs of living material were remarkably well reproduced in this periodical.

All these investigations proved to be so convincing that even P. M. Zhukovsky was obliged to state in his textbook, *Botany*, that the old notions of sperms as being naked nuclei must be abandoned, and that apparently male gametes in angiosperms are cells.

Everything seemed to be in order. Several other vital observations were published after the war. In recent times, however, probably on account of the aggressive attitude of the Morganists, sperms have again begun to be described as naked nuclei. But the research workers concerned employed an imperfect technique, involving simultaneous fixation and staining with acetic carmine.

It has been demonstrated long ago both in our own and in foreign literature that this method tends to destroy many of the delicate structures of the cell. It was necessary once again to show that the fixing and staining technique was responsible to a considerable degree for introducing errors into these investigations. To make this point clear, I have carried out a comparative investigation of vital and fixed material, which I have described in a recent paper shortly to be published. I have elaborated a fixing method so perfect that I have been able to obtain cell-shaped sperms in the famous *Lilium martagon* on fixed material, something that Navashin failed to achieve.

I have succeeded in establishing once and for all that the reason for sperms being described as naked nuclei is merely an imperfect technique. I ask for permission to present to the Presidium of this conference some photomicrographs and drawings, taken both from vital and fixed material. All of them are combined with *Lilium martagon*—the famous material for cytological investigation.

It should be pointed out, however, that the appearance of the works referred to was due not only to the errors of inexperienced investigators—conflicting principles were involved here.

I take the liberty of quoting a review, true, an anonymous one, but, as the saying goes, the beast can be identified by its claws. It is a review of a paper I have written and which I was simple-minded enough to send to the editorial office of

the *Doklady of the Academy of Sciences of the U.S.S.R.* I shall quote a few lines of it.

"To the author, however, it seems that in the case of individual development everything has to undergo development, up to the last molecule. She refuses to understand that in the course of individual development only that undergoes development which is not transmitted from generation to generation."

"Proceeding still further, Kostryukova, along with the school of naive Lamarckists, wants the structure of the cell to be determined by the nutrient substance resulting from lengthy transformations which are due to development. Hence, of course, the next step is 'inheritance of direct adaptations.' "

This forthright statement enables us to understand why the Morganists hate Lamarck so much, why they are waging a struggle against him as though he were alive, although he has been dead 120 years. The explanation is that Lamarck understood that which they still fail to understand, namely, that development proceeds on the basis of interaction with the environment.

Let me quote one more paragraph from this review. It is a cry from the depths of the soul of one who is very sorely tried by the work of Michurin's disciples:

"I believe that the article is of no scientific interest; what is more, it may mislead the uninformed reader. It is highly regrettable that Kostryukova has already published a number of articles which spread confusion and disseminate obsolete ideas with extraordinary bombast."

My article has been published at greater length in issue No. 2 of the periodical *Agrobiologia* for 1948.

Permit me to conclude on this. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician S. N. Muromtsev.

Academician S. N. Muromtsev. One of the speakers here called this session of our Academy a noteworthy one. That is undoubtedly true. It is clear to everybody now that this session marks the utter ideological defeat of Weismannism-Mendelism in our country. This situation has been welcomed with great satisfaction by all progressive agrobiologists, by all progressive

practical agriculturists. It is this, undoubtedly, that constitutes the most important feature of the present session.

It is no less evident that the present session marks the beginning of a new, unprecedented stage in the development of Soviet Michurin genetics, of the still more intensive and extensive application of the doctrine of Michurin and Lysenko to practical farming in our country.

I do not intend to dwell here on the two irreconcilable conceptions that exist in modern biology, namely, the doctrine of Michurin and that of Mendelism-Morganism. It has already been done with sufficient precision and clarity by many of those who have preceded me, and particularly by Academician Lysenko.

I wish in my remarks to demonstrate that we are dealing not only with purely theoretical differences between representatives of Michurin biology and advocates of Mendelism-Morganism. The contradictions are far more deep-rooted. Those that exist in the theoretical interpretation of the key problems of modern biology are based upon sharp differences in the general approach and in the methods of solving scientific problems. What is more, I think that this, just this, is the main source of the theoretical differences themselves.

That is why I think it necessary to dwell upon the question of the basic differences in the general approach to science and practice, which are typical of the representatives of the two tendencies in biological science.

Under Soviet conditions, what characterizes our progressive scientists is that they approach the solution of theoretical and practical problems as innovators, in creative, revolutionary-critical fashion. We Soviet scientists, Soviet folk are taught this approach to problems by those great coryphaei of science, Lenin and Stalin. There is no need to quote the views expressed by Lenin and Stalin on the subject, for everybody present is aware of them. Indeed, the entire life of our country, the entire practice of our socialist construction in all spheres of industry and agriculture is the tireless creative path of unparalleled progress.

Scientists, technicians and agriculturists who have learnt to approach their work creatively, as innovators, and actually do so, are truly progressive people who enrich both theory and practice with great new achievements.

Those scientists, on the other hand, who approach the solution of theoretical and practical problems dogmatically, as textmongers, are inevitably sterile in their practical work, while in theory they are backward, and then reactionary.

In the sphere of scientific problems, the progressive Soviet scientist is the one who approaches theoretical problems of great import not as an arm-chair philosopher, but as one who, from the very outset, draws on an extensive range of practical experience. Boundless possibilities, the like of which do not and cannot exist in bourgeois, capitalist countries, are provided by our social system for those who employ such a method of solving scientific problems. And there can be no doubt that Academician Lysenko's achievements in both theory and practice are due, apart from his personal qualities, primarily and mainly to the conditions under which he works, conditions provided by our Soviet socialist system. No scientist, in any other country but ours, is able to enjoy such conditions.

Academician Lysenko is a biologist-theoretician, who is contributing to the further progress of the doctrine of Darwin and Michurin. At the same time he is a no less talented and energetic organizer of the masses, who is supported in his work by millions of collective farmers. Just try to point to a scientist of that type in a bourgeois country! Just try to point to any other country where scientific problems are solved in this way! There is no country in the world where agrobiological science has been able in so short a period of history to enrich practical farming with such a large number of new methods of remaking nature for the benefit of man.

Nevertheless we must constantly bear in mind that our Soviet system not only guarantees us special facilities for our scientific labours, but also demands of us, scientists, that science fulfil its responsibilities to our country.

Scientists who are accustomed to think in scholastic fashion, to work in the seclusion of their studies, adopting a servile attitude to traditional, obsolete propositions in science, are barren in the sphere of practice.

Such scientists, who engage in science only for its own sake, who engage in experiment only for its own sake, lose the capacity for solving the problems that answer the country's needs. What is more, in the long run they also become incapable of

understanding the current economic problems that face their country. Hence the practical sterility of their scientific research, the stagnancy and meagerness of their theoretical thought, hence their increasing lag behind genuine creative science, and their failure to render the State the practical assistance of which it stands in such need.

Isolated in their studies and laboratories from the practical work that is going on, these scientists have proved to be highly prolific in one thing alone, namely, in writing stout, scholastic, speculative volumes, stout monographs of a descriptive character. Unable to back up their Mendelist-Morganist views with convincing experiments or results of practical importance, formal geneticists have in their impotence slipped into a position of utter lack of principle. They have taken the line of denying that there is anything of scientific value in the works of Lysenko, which they try to place on a par with mere experimentation. They pass over in silence the works and the name of the great reformer of nature, Ivan Vladimirovich Michurin. They want to close down progressive Soviet biological science, to discredit our methods of solving great practical problems, the methods advanced by Michurin and Lysenko of which we ought to be proud, if we have not lost all sense of Soviet patriotism.

Only people who have been driven into a fury by their own sterility, people whom I take the liberty of calling politically backward, can fail to understand this and attempt to ring down the curtain on our progressive biological science. But that is beyond their strength.

There is no reason to doubt that if the representatives of the Mendel-Morgan school fail to understand the need for a creative approach to the solution of the problems facing biological science, if they fail to appreciate their responsibility to the field of practice, they will be left behind not only by socialist science, but also by the practice of socialist construction in this country.

I would like to make some remarks concerning the speech made by Professor Rapoport, in the part dealing with microbiology. One of the speakers—Academician Perov, if I am not mistaken—said that, in order to discuss a topic, one should at least possess a superficial knowledge of it. I would like to re-

mind Professor Rapoport of this, too, in connection with his excursion into the realm of microbiology.

Really, how could Professor Rapoport say that microbial cultures with reduced antigenic systems are used for inoculation? What is the use of inoculation practised with such cultures? Who needs them? Just the opposite is true: microbiologists are trying to obtain microbes endowed with increased antigenic activity.

Moreover, what was Professor Rapoport trying to prove when he cited the examples of vaccination against rabies and tuberculosis? Pasteur was the first to obtain races of microbes with hereditarily-reduced virulence, which could be used to protect human beings and animals against contagious diseases. He obtained them precisely by changing the conditions of the habitat of the causative microorganisms of these diseases. Pasteur proved the existence of an indissoluble interrelation between the microbe and its environment. All subsequent investigators followed and continue to follow the same lines. Moreover, it can most reliably be stated that all the most important achievements in medical, soil and industrial microbiology have been the result of the interaction between microorganisms and their surrounding medium, effected, more often than not, spontaneously, or not entirely deliberately. In no other world of living bodies can one find so obvious, so close an interconnection between the organism and the medium as in the case of that unicellular body, the organism of a microbe.

The struggle between two trends in biological science is manifest not only in agrobiology. A violent struggle for the cause of Darwinism, now dying down and now flaring up again, has gone on in the field of microbiology ever since the days of Pasteur. A huge amount of evidence bearing on the variability of heredity, the phasic development of microbes, and interspecific competition has been accumulated by microbiologists. This science is awaiting its own Lysenko to liberate it from that main obstacle to its development, namely, the metaphysical law of species constancy advanced by Kohn and Koch, and the autogenetic interpretation of data accumulated on variability and heredity in microbes.

Finally, what did Professor Rapoport mean when he said that the electronic microscope has made it possible to observe

bacteriophage? In so far as I was able to understand, he considers this to be decisive evidence of the point that phages are living organisms. But firstly, not everything that we see is alive, Professor Rapoport; and secondly, the corpuscular nature of phages has, just imagine it, been proved long ago by purely biological methods, and has long been common knowledge to all who are acquainted with the phage problem.

There is only one thing, Professor Rapoport, that we would like you, cytologists and cytogeneticists, to understand. We have no objection to cytological investigations of the protoplasm and the nuclear apparatus in sex, somatic or any other cells, inclusive of microbial cells, a study, by the way, in which cytologists of the Academy of Sciences of the U.S.S.R. are deeply engaged. In spite of what you say, we recognize these modern methods of research as quite indispensable and highly promising. But we are energetically opposed to the Weismannist, unscientific theoretical premises on which you base your approach to your cytological investigations. We object to the problems you intend to solve with the aid of these methods, we are against the unscientific interpretation of the results of your morphological investigations, isolated as they are from progressive biological science.

This is where we differ. This is also one of the specific examples of how, as I stated at the very beginning of my speech, we differ in principle as to method and general approach to the solution of scientific problems. And if you do not realize the difference, Professor Rapoport, your cytogenetic research will prove as sterile as the entire school of formal genetics. (*Applause*.)

Academician P. P. Lobanov. Academician B. M. Zavadovsky has the floor.

Academician B. M. Zavadovsky. Comrades! In the first place, I must explain to those present why I have not considered it advisable till now to take the floor at this session. I think that the conditions under which the session has been organized have not been quite normal, for all those deservedly placed—and particularly those undeservedly placed—in the category of Weismannist-Mendelists have not been given adequate facili-

ties to prepare themselves and to give free and full expression to their views.

Suffice it to say that only on July 30 when I arrived in Moscow on my way from one sanatorium to another, did I receive official intimation that this session was to take place, although the Academy and its board of management were well aware of the fact that I was undergoing treatment at Kislovodsk.

I will not deny that informally I had learned from Comrade V., who was likewise undergoing a cure at Kislovodsk, that arrangements for such a session were in hand. It is strange, however, that I, who am accused of such mortal sin, was neither allowed to acquaint myself with the theses of the address, nor informed in advance of the convocation of the session.

My view was that it would be more normal, more reasonable, if at this session which, as I fully understand, is to determine the line of development of biological science and form an estimate of its present condition—better facilities were provided for those who are taking part in building up Soviet science, instead of creating an atmosphere of premature defamation, as has been done, in particular, in the columns of the *Literaturnaya Gazeta.*

The article I have read in today's *Pravda* makes it incumbent on me to express my views at this session. To be frank, this article relieves me of all the doubts or hesitations that I have experienced.

Passing to the essence of the problem, I must enumerate first of all the points on which I agree with T. D. Lysenko and with the main tendency expressed here by other comrades, and secondly, dwell upon the points on which I do not agree with them.

I agree with the whole line of attack which is being pursued against formal genetics. In this regard I have no need to change my views, for as far back as the year 1926, in my book *Darwinism and Marxism* I came out against formal genetics. I did the same in all my subsequent public statements, including those in the year 1936, when I was the only member of the Agricultural Academy who, along with T. D. Lysenko, attacked formal genetics.

I need therefore make no change in my negative attitude towards Weismannism, Mendelism, and formal genetics.

I am all the more entitled to protest since, although everybody is aware of my works and my public statements, the Soviet public is actually being misled into believing that I am an adherent of formal genetics, although no evidence is brought in support of this claim. The only reason this accusation is made against me is that I am at variance with T. D. Lysenko on a number of other points. I feel myself entitled not only to protest against such unfounded accusations, but also to disclose the profound disagreements that exist between myself and T. D. Lysenko.

All my further remarks about my disagreements with T. D. Lysenko will be made in fulfilment of my duty as a Party member, in order more correctly to inform the Party and Soviet authorities and the entire Soviet public about the real state of affairs and requirements of Soviet science.

I am an ardent adherent of the Michurin trend in science, and I have said so on more than one occasion. I have combatted the errors of formal genetics which in this sphere have been duly analyzed by me in sufficient detail and exposed in a number of my papers. Anyone who is sincerely anxious to be guided by facts and the truth will be able to find these papers and statements of mine. That is why I see no need to repeat what has already been said in this connection by T. D. Lysenko and myself.

Finally, as a Darwinist, I also agree with T. D. Lysenko and other comrades who have spoken here as regards their general attitude towards the tremendous, decisive role played by environmental conditions and their influence in the processes of speciation and variety formation. Nevertheless, there still remain a very large number of problems of paramount significance on which I do not agree with T. D. Lysenko.

I therefore consider it necessary to deal here with the points on which I am at variance with him.

In the first place, as I have already pointed out, I maintain that his address and the speeches that followed it mislead the Soviet public as to the state of affairs and distribution of forces in Soviet biological science. We, scientists, are prospectors not only in practically applying our experience and

knowledge so as to discover the minerals or other wealth of which our Socialist Motherland abounds. We are also prospectors in the sense of getting correct bearings as to the distribution of forces in our science. That is why I think that Comrade Lysenko is committing a serious mistake by misleading people into believing that in biological science only two fronts or two trends are at work aiming at the solution of the problems of Darwinism. Every biologist is aware of the fact that three tendencies exist in the theory of Darwinism, in the theory of evolution. The first of them is represented by Darwin and Timiryazev: this is a line of consistent Darwinism. I ask you carefully to consider and analyze the very essence of the problem, and not to make play, so to speak, with what may be groundless terminological errors.

I have not had the opportunity to answer Comrade Mitin who, in the *Literaturnaya Gazeta,* disputed the term "orthodox Darwinism," and to tell him that I do not insist upon this term but consider it more correct to speak of "consistent Darwinism," or else, simply of the Darwinism professed by Darwin himself and by Timiryazev.

And I have grounds for asserting that those who continue to make play with these verbal, petty forms of argumentation, are perfectly well aware that that is not the main point. But neither Comrade Lysenko nor any of his adherents have yet given me an answer on the main point at issue. Am I right in maintaining that there are three, and not two trends in the theory of evolution? I undoubtedly am. This evaluation of the real state of affairs was always maintained by that great scientist, K. A. Timiryazev, to whose name speakers have made such frequent appeal.

I maintain that if the adherents and admirers of T. D. Lysenko's talent not merely revered Timiryazev, but read him as well (and some of them have forgotten to do so), they would not appeal to the name of Timiryazev. Running through all his works is the idea of a struggle on two fronts: a struggle against the errors of Neo-Lamarckism, the oversimplification of the problem of evolution, on the one hand, and against the errors committed by the Weismannist-Mendelists, on the other hand. But Timiryazev could not discuss such new variants of Weismannism as formal genetics

and autogenetics, since they made their appearance after his death.

Let me quote just a short sentence from Timiryazev's "The Significance of the Revolution Brought About by Darwin":

"Desirous of displaying independence of thought, the authors who succeeded him [Darwin—*B. Z.*], merely lapsed into the narrow one-sidedness (Neo-Lamarckists and Weismannists) that was always absolutely alien to Darwin."[1]

Such is the real state of affairs, and it is typical enough of what has occurred in the history of the development of the evolutionary movement. We have been taught by Comrade Stalin to base ourselves on the experience of history, and there is no reason to indulge in the arbitrary "creation" of a history of Darwinism that does not conform to the facts.

The Soviet period has seen the development of the teachings of Darwin and Timiryazev on the basis of the experience gained from scientific and philosophical discussions; these have contributed considerably to making our attitude towards Darwin's theory a more normal and precise one. This struggle waged on two fronts for the general line of the teachings of Darwin and Timiryazev, was raised to a still higher level in the light of the experience gained by our Party in a similar ideological struggle on two fronts in all spheres of our social and political life.

Permit me to hand over to the Presidium a diagram I drew up several years ago on this problem. It characterizes the basic principles of Darwinism, on the one hand, and on the other, those of Neo-Lamarckism and Neo-Darwinism, as being two distortions of Darwin's real theory. This diagram shows each of the three trends, representing three finished systems of views; the only correct one, the one that corresponds to the spirit of Marxism-Leninism, is that of Darwin and Timiryazev, cleansed of a number of minor errors in the light of Marxian dialectics.

I believe, comrades, that we are committing a serious mistake, and are misleading authoritative bodies, by insisting that only two lines, two trends exist in Soviet biology:

[1] К. А. Тимирязев, *Сочинения*, том VII, стр. 250-51.

the doctrine of Lysenko, called the Michurin trend, and that of the formal-genetic Weismannist trend, while all who think otherwise and are bold enough to disagree with Lysenko, are allocated wholesale by Lysenko's adherents to the odious category of "formal geneticists."

This makes it my duty to declare that I must speak in defence of the line that has not yet been rejected by the public of this country.

Voice from the audience. But when were you told so by the public of this country, when were you empowered to say so?

B. M. Zavadovsky. It is not I who was empowered to say so, but our Soviet science as a whole.

The second point on which I disagree with the line of the presidential address, is the evaluation of the attitude of Timiryazev and Michurin to Mendelism. The Soviet public at large who do not read Timiryazev's works in the original are being misinformed. In all his numerous statements on the problem, the great Russian scientist and Darwinist stressed the point that he drew a distinction between "Mendelism" and "Mendelianism." By Mendelism he understood the total of factual scientific data and methods that are devoted to the study of the chromosomal-nuclear mechanisms of heredity. By Mendelianism Timiryazev understood the idealistic and reactionary interpretations and conclusions that have been fallaciously drawn from these valuable scientific data, if not by all bourgeois and our own Mendelists, then at any rate by most of them, who in the past made them the basis for their attacks on Darwin's theory. But even in these conditions Timiryazev was able to discern the sound core of facts from the husk of reactionary anti-Darwinian generalizations.

I am very sorry to say that, although many of those present here are aware of this real attitude of Timiryazev, for some reason or other they do not consider it necessary correctly to inform the public on the matter.

We need not look for material dispersed among the works of Timiryazev and Michurin. I shall merely cite a statement by our philosopher, Mitin, who in a summary of the results of a discussion on breeding and genetics organized by the

editorial board of the periodical *Pod Znamenem Marxisma* and published in issue No. 10 for the year 1939, wrote the following:

"Mendel undoubtedly revealed certain laws governing the inheritance of a number of definite characters: the phenomenon of segregation in hybrid progeny, a certain mathematical regularity of this segregation, a relative independence in the inheritance of certain characters. Subsequently, the phenomena discovered by Mendel in the sphere of heredity, were associated with processes taking place in the cells of the organism, particularly in the sex cells.

"In evaluating all these Mendelian regularities, which are undoubted as partial rules, we wish to adhere, and do adhere, to the viewpoint of Timiryazev and Michurin. For us Timiryazev and Michurin are authorities in this field. As really eminent representatives of science, they were successful in giving correct answers to the question of the scientific importance of Mendel's discoveries in the sphere of the study of heredity.

"Timiryazev's views on Mendelian laws have been cited here. They are manifold. Being an outstanding scientist Timiryazev approached these laws in by no means a one-sided fashion. He opposed any universalization of these laws, their transformation into universal laws of nature, the replacement of Darwinism by Mendelism. Timiryazev did not oppose the Mendelian rules, but the 'Mendelianists' who, without any grounds for so doing, turned Mendel's discoveries into an entire revolution in science, turned the laws discovered by him into universal laws of nature, and (out of class and all sorts of other considerations), began either to place the name of Mendel alongside that of Darwin, or to counterpoise Mendelism to Darwinism.

"While opposing such anti-Darwinists, 'Mendelianists,' Timiryazev used, at the same time, to point to the positive significance of Mendel's discoveries in solving certain partial problems of heredity 'Thus, inasmuch as it justifies itself, Mendelism merely serves to support Darwinism, eliminating one of the most important objections ever advanced against it.' (K. A. Timiryazev, *Charles Darwin and His Teachings,* p. 263, 1937.)

"Such were the conclusions drawn by Timiryazev.

"K. A. Timiryazev speaks of 'successes in the study of certain partial cases of heredity (Mendel and his numerous admirers)...' (K. A. Timiryazev, *Charles Darwin and His Teachings,* 1937, p. 270.) Here is a correct, precise, scientifically objective estimation of Mendel and Mendelism, an estimation which is free both of the one-sided predilection for Mendelism, and of the wholesale denial of its importance in the science of heredity.

"Speakers at this conference have cited numerous quotations from Timiryazev's works, each of them taking some one aspect of the many-sided treatment of the problem by Timiryazev. Some of the speakers cited such quotations as emphasized the importance of Mendel's discoveries; Comrade Prezent picked out statements in which Timiryazev criticized the predilection for Mendelism. But neither the ones, nor the others were able to understand the genuinely scientific, objective and many-sided estimation of Mendel given by K. A. Timiryazev. I see no reason why we should take from Timiryazev's works only one part or one side of his treatment of the question. I see no reason why we should not adopt everything Timiryazev had to say on this question.

"Let us analyze Michurin's attitude towards this problem. To all those who are anxious to be Michurin's followers and really to develop his theory, his doctrine, to be the vehicle of his ideas and methods, the material in Michurin's writings should form the basis of their work....

"If the comrades who referred in their speeches to Michurin find that some of his ideas have become obsolete, let them say so outright. But I am convinced that it is just Michurin's approach to the problem of the Mendelian laws that has not become obsolete and that is the correct one now.

"Reference was made here to Michurin's letters written in 1914 or 1915, in which he referred ironically to the Mendelian laws as the 'pea laws.' But if we take Michurin's fundamental work, *Results of Sixty Years' Labour,* we find the following statement:

" 'Thus in hybrids between pure species of rye, wheat, oats, peas, millet, etc., I consider the "phenomenon of segregation

of the parent types" to be quite possible. The Mendelian laws are applicable here in many details.'

" 'I by no means deny the merits of the Mendelian law. On the contrary, I merely insist on the need to introduce amendments and addenda into it, for it is evident to everybody that his calculations are not applicable to cultivated varieties of fruiters, for when crossing separate varieties of them, the structure of the hybrids is not due to the hereditary transmission of the characters of the direct and immediate progenitors, but in most cases of those belonging to the ancestors of the parent plants, unknown to the originator. In addition, it is due to the influence of external factors, which not infrequently introduce the utmost perturbation into the organisms of the hybrids not only at the initial stage of seed formation after the cross, but also for several years during which the hybrids, manifesting sport deviations, develop and grow to full maturity. It should be added that the greater part of these influences of both internal and external factors are beyond the control of man.'

" 'When investigating the application of Mendel's law to the hybridization of cultivated varieties of fruit plants, I recommend that, for a beginning, the investigation should be confined to observing the hereditary transmission of one of the two characters, just as Mendel himself did in his work on peas. I find it particularly useful to indicate a few of the best and in every way exemplary experiments in hybridization.

" 'In these examples the choice of the parent plants, i.e., of the male and female plants, provides wide possibilities, from the very outset, for carrying out the necessary observations with ease and precision; this can be done by using the colour and shape of the hybrid seed, the intensity of the colouration of the cotyledons, later by using the colour of the leaves, sprouts, blossoms and, finally, the shape, structure and colour of the fruit. Occasionally there also occurs a correlative remodelling of structure, analogous to the ones referred to above, and resulting from the influence of some sharply expressed characters, which have remained in a recessive condition till a definite moment.

" 'Here there is great scope for applying the whole Mendelian calculus to the entire complex of characters of each hybrid.'

(I. V. Michurin, *Results of Sixty Years' Labour,* 1936, pp. 24, 33, 37.)

"Is there any contradiction in Michurin's statements, between his allusion to Mendelian laws as 'pea laws,' on the one hand, and his admission of the possibility of their application, on the other hand? I do not find any contradiction. When he speaks of 'pea laws,' he means such cases where Mendelian laws are transformed into universal laws of nature."[1]

Comrades, I quote this excerpt from *Pod Znamenem Marxisma,* No. 10, for the year 1939. I entirely agreed and still agree with the ideas expressed there. But I ask Comrade Mitin, on which occasion did he misinform the public, when he spoke of the existence of two trends in biological science? Was it when he wrote that article, or now when he draws a false picture of the actual state of affairs, and so leads us to believe that only the second interpretation of the theory of evolution existed then and exists now?

It is not right, comrades, to indulge in wholesale criticism of our Mendelists, as has been done here not only in the presidential address, but also in some of the other speeches. The level of the arguments being employed here in the main, is the same as that of the discussion of the 1931 period, and I would say that I have no grounds for objecting to these arguments, which I used myself and still adhere to.

But, comrades, everything develops and grows, and the errors of the representatives of the Mendelian doctrine in our country are being rectified to a considerable degree. They are adding valuable achievements to the treasure store of our Soviet science and practice.

Accordingly, it should be a matter not of driving Mendelian genetics out of Soviet science, but of further re-equipping and re-educating those of our cadres who continue in some degree to remain in the grip of the old errors of Mendelianism and formal genetics.

I think that this front of our struggle still remains; yet I had the right to expect that comrades speaking here would give more differentiated support to those Mendelists who have

[1] М. Б. Митин, „За передовую советскую генетическую науку", *Под знаменем марксизма,* 1939 г., № 10, стр. 160-62.

already freed themselves of their old errors and, by way of helping them, indicate in a differentiated and business-like way what they should rid themselves of (and there are still many things of which they should rid themselves) instead of defaming them, and allocating almost to the enemy camp all scientists who are engaged in studying Mendelian genetics and in using it in the interests of our national economy.

I fully agree with the need for utterly routing idealistically-mechanistic conceptions, but we are bound to make use of the sum-total of scientific experiments accumulated by the Mendelists. We are bound to make use of the method of polyploidy and that of intervarietal crosses of Indian corn, a method that has brought enormous wealth to the United States of America. We must not throw these achievements overboard, must not throw out the baby with the water.

I have heard it said here, that the effect of colchicine is like the blow of a stick. Let us take a broader view of things. Suppose we are fattening hogs, or are trying to raise a readily-fattening breed of swine. In that case, from the standpoint of the animal's benefit, to transfer it to natural surroundings is tantamount to killing it or is a specific way of hitting it with a stick. But sometimes, when it is in man's interest, one can and should deal such a blow, and there is no need to shrink from such methods.

I now hear arguments such as that it is not important to conduct a struggle on two fronts in biological science. In an article in the *Literaturnaya Gazeta* published, apparently, with the approval of the editorial board, for there is no stipulation to the contrary, V. N. Stoletov condemns my suggestion to take account of the tried and tested principle of a struggle on two fronts, because, he alleged, it is a proposal to support the "third position" which we are exposing on the international arena, particularly in the tactics of Blum and other social-traitors, as being the platform of a "political swamp."

This is a grave accusation, indeed. But, unfortunately, some comrades here are trifling with words, without understanding that in training our cadres we must approach the ideological struggle on the international arena and that inside this country in different ways.

It seems to me that those comrades who identify the two things fail to understand that the struggle on the international arena and that within our country assume qualitatively entirely different forms. When it is a matter of the struggle on the international arena, where two enormous bulwarks confront each other, one representing the imperialist, anti-democratic front, and the other, the democratic, anti-imperialist front—there can be no middle, "third" sound line. All who prove to be between these two bulwarks of the class struggle really are in the position of being conciliators and social-traitors. Here all conciliation must be ruled out altogether, and we have to learn to support the struggle against this conciliatory outlook.

But where Socialism has been victorious there exists but the one general line of our Party, that of Marxism-Leninism, and the problem of conducting a struggle on two fronts against anti-Party right and left-wing deviations, and against scientific and philosophical errors—against the mechanistic vulgarization of Marxism, on the one hand, and against Menshevistic idealism, formalism and metaphysics, on the other.

We are entrusted with the responsible task of helping those who are engaged in the theory and the practice of biological and agricultural science to advance from the level of elementary natural-scientific materialism, to that of conscious materialistic dialectics. The errors of certain formal geneticists are, in my view, more dangerous, for in most cases they serve as a channel through which bourgeois idealistic and metaphysical influences penetrate into our biological science.

It has never been said by our Party that the problem of conducting a struggle on two fronts has been withdrawn from any field of political or ideological life.

Now I ask you: can we be satisfied with the analysis of the condition of biological science given to us by T. D. Lysenko in his address?

No, we cannot. We heard him speak of an extensive fighting front and of the rout of the formal-genetical errors. But where is the fighting front against mechanism?

Voice from the audience. In the same place.

B. M. Zavadovsky. Now that is what I fail to understand, and I would like somebody to enlighten me on the point.

Nothing was said in Lysenko's address about the fighting front against mechanism; yet it exists, and to ignore it means to disarm our Party and Soviet public opinion as regards the struggle on this sector.

Voice from the audience. Where, in your opinion, is this front?

B. M. Zavadovsky. I shall deal with that a little later.

Where do I disagree as regards the tactical problems of the struggle against that same front of formal genetics? I do not agree with the wholesale defamation and allocation to this front of people whose services to the struggle for disarming this front are great. It is wrong to indulge here in the wholesale defamation of such eminent Darwinists as Academician I. I. Schmalhausen and his followers.

I see a profound contradiction between the line being pursued by our Party for raising the prestige of our Soviet science, and that being followed in the *Literaturnaya Gazeta* and in a number of other public statements which condemn wholesale those Soviet scientists who have not joined the chorus of admirers of T. D. Lysenko, or defame them, as in the case of Academician I. I. Schmalhausen, merely for daring to voice some disagreement on questions of interspecific competition or for committing some secondary errors. From the point of view of mobilizing Soviet science as a whole, such an approach is not in the true interests of our work. Who, in fact, is Academician I. I. Schmalhausen? He is one of the disciples of Academician Severtsov, whose school of evolutionary morphology is in many respects on a par with that of I. P. Pavlov's school of physiology in this country. Both Severtsov and Schmalhausen are the continuators of the classical Soviet Darwinism created by the brothers Kovalevsky and I. I. Mechnikov, whom we extol, as against the contrary efforts of bourgeois reaction. From my point of view, Academician I. I. Schmalhausen is a brilliant successor of theirs. He should, of course, be criticized, but Academician T. D. Lysenko should be criticized, too.

Let me pass to the highly important problems to be solved by this session of the Academy, relating to the disclosure of the real sources of Darwinism, that should serve as the basis for our Soviet biological science. On what should Soviet Dar-

winists base their work? On all the diversity of methods of investigating natural phenomena.

The main sources from which Darwinism originated were, on the one hand, the methods of breeding, and on the other hand, the methods of evolutionary morphology and ecology. Ought we to close down these two trends? I think not. Academician I. I. Schmalhausen is recognized as a brilliant worker in the field of evolutionary morphology not only by people in this country, but also in progressive scientific circles all over the world. But the fact that Academician I. I. Schmalhausen is mistaken in some of his statements—and at the conference which took place in November 1947 I declared myself at variance with some of the views expressed by him—does not entitle us to pile up indiscriminately all these various trends of biology, and to place them all into one bag entitled—formal genetics. That is falsification.

Under Soviet conditions new trends have come into being which are splendidly developing Soviet Darwinism on new foundations. I have in mind the new evolutionary physiology whose foundations were laid by I. I. Mechnikov, further successfully developed by Academician I. P. Pavlov and now being developed by Academician L. A. Orbeli and a number of other Soviet physiologists. Evolutionary physiology constitutes an active approach to the nature of organisms, to the problem of subordinating it to our influence. In the elaboration of this trend first place belongs to I. V. Michurin. I have made this admission repeatedly, and not under pressure from this session, where the discussion is somewhat one-sided. There is no place where the achievements of Michurin and Lysenko have been so fully displayed as in the Timiryazev Biological Museum; you will not find such a complete exhibition of Michurin's work anywhere else.

A. A. Avakian. Just try not to display them!

B. M. Zavadovsky. At the same time, however, I assert that the Michurin trend cannot cover, exhaust, eliminate the trends that exist alongside it.

I doubt whether anybody can seriously suggest that the Michurin trend be applied to animal organisms, particularly in the direction followed by T. D. Lysenko in respect to the vegetative hybridization of species. Apart from creating chi-

meras—such as butterflies with different wings—no vegetative hybrids in animals have yet been proposed. Give us concrete instructions and proposals as to how to apply the methods of vegetative hybridization (T. D. Lysenko's prime symbol of faith) to the animal world.

I. I. Prezent. Why should others do the thinking for you?

B. M. Zavadovsky. Physiologists and animal breeders are probably not talented enough. Help us, you men of talent and admirers of the talented ones, give us effective aid!

But there are other methods and procedures which cannot be sacrificed or rejected in either science or practice merely because they do not come within the purview of T. D. Lysenko.

What other trends are there? On the basis of my personal experience as a Soviet Bolshevik biologist I maintain that the methods of polyploidy, which have been applied by Sakharov for obtaining his new varieties of buckwheat, or by M. S. Navashin in creating his high-yielding varieties of kok-saghyz, can occupy a well-deserved place in our socialist farming. And there is absolutely no need, for the sake of glorifying the work being done by T. D. Lysenko, to ruin or exterminate these trends. Sakharov, Navashin and Zhebrak should be criticized where they commit theoretical mistakes. But when I heard an appeal made here to rout the Mendelist-Morganists, to deprive them of facilities for working, I became absolutely clear as to the great damage that would be done to the national economy by such action.

Darwinism includes a trend that is based upon experimental-physiological methods of discovering the factors regulating vital functions. I shall not adduce all available examples. I shall only cite the hormonal-chemical method of directing processes of propagation which has already been recognized as a means of stimulating propagation and combatting barrenness in cattle. Let me also point to phytohormones in plant breeding. All these methods are penetrating with difficulty into the national economy for the sole reason that T. D. Lysenko has not yet included them within his sphere of influence and till now has offered considerable resistance to them.

Is such an approach to the analysis and direction of research in the field of Soviet biological science a correct one

and is it of benefit to the State? That sort of approach transforms State tasks into monopoly tasks. Nobody has yet given practical proof that the methods of polyploidy have not justified themselves. Wheat and rye varieties with which millions of hectares of land are planted were created by the geneticists A. P. Shekhurdin, P. I. Lisitsyn, and P. N. Konstantinov.

I. I. Prezent. On what basis?

B. M. Zavadovsky. On the basis of the manifold and many-sided application of all trends and methods that are created by Darwinian science.

I. I. Prezent. That's not clear, Comrade Zavadovsky.

B. M. Zavadovsky. And they do not conflict with the Mendelian laws, but are frequently based on them.

I believe that this narrow, restricted, one-sided line of defaming not only methods, but also the people whose work does not follow the line encouraged, is impermissible.

I was very sorry indeed to listen here to the speech of Comrade Muromtsev who, I think, spoke the way he did merely because it seemed to him that it was called for by the situation. My reason for thinking so is that there are no reasons for Comrade Muromtsev, with all his experience—good, positive experience—making a speech here in which he stigmatizes forms of work which he has apparently not studied or come to know sufficiently.

Let me pass now to the question that particularly worries me. To my profound distress and that of many, many others the events that are taking place here are, in a number of instances, absolutely at variance with the views that were expressed by Darwin and Timiryazev.

Comrades, we ought after all to be clear as to our duty to employ correct concepts and not to disguise the correct world outlook of anyone of us by presenting it in unsuitable forms, as, unfortunately, is being done quite often.

I have presented my diagram which I ask to be refuted by businesslike arguments. It clearly shows that there are three trends in existence, viz., Darwinism, Neo-Darwinism and Neo-Lamarckism.

Darwin's teachings comprise a harmonious system, whose separate parts (apart from certain errors) are organically ad-

justed to such a degree that they cannot be severed one from another.

Now listen to what Comrade Lysenko says about his attitude towards Darwinism in his work *Natural Selection and Intraspecific Competition.*

"As is well known, Darwin and Darwinists point to the universally observable great discrepancy between the number of embryos of organic forms that appear, and the number of organisms that attain mature or old age. For example, in plants, insects and fishes the number of organisms of mature age is hundreds or thousands of times less than that of the embryos engendered. But I consider the interpretation of this phenomenon as given by Darwin and subsequently repeated by many (if not all) Darwinists, and which is based upon intraspecific competition, to be a wrong one."[1]

Everything is clearly stated here. For ten years, since the time when Comrade Lysenko was just beginning to abandon Darwinism, but still called himself a Darwinist, I have been trying to sound the alarm about the appearance of steadily accumulating errors in his views. In the above quotation, however, Comrade Lysenko subjectively admits his disagreement with Darwin and with most, if not with all, Darwinists.

Who has given anybody the right to invest the formula of Darwinism with a content that contradicts this doctrine? Things should be called by their names, Comrade Lysenko. In that case, however, it is Comrade Lysenko's duty not to employ dictatorial methods or engage in oracular utterances in order to compel us to change our attitude to Darwinism, but to substantiate his new views comprehensively, to show why and for what purpose we should cease to expose Neo-Lamarckism as an anti-Darwinist, anti-Marxian doctrine.

This is a very serious problem, and as yet Comrade Lysenko has obviously failed to solve it. But then let us recall the words of K. A. Timiryazev, to the effect that Darwin thought for 20 years, before deciding to publish his doctrinal system. Why, then, has Comrade Lysenko hastened, on the basis of a few isolated facts, to create the mental confusion that has now arisen in universities and secondary schools, as

[1] Т. Д. Лысенко, „Естественный отбор и внутривидовая конкуренция", *Совхозное производство*, № 1, 1946 г., стр. 12.

well as among practical agriculturists, and resulting in the frequent statement that if we are to give the name of Darwinism to the things taught by Comrade Lysenko, then we are entering into conflict with our consciences as scientists and teachers. Let us, then, frankly declare why we are renouncing Darwinism. Can we adopt the new principles advanced by Comrade Lysenko? No, we cannot, for this system is growing into a system of grave fallacies.

In the same work by Comrade Lysenko, I have found several places where he says clearly and frankly that he does not admit the category of chance as the form of functioning law accepted by Marxian dialectics:

"If overpopulation can be observed in exceedingly rare cases, it occurs in a purely accidental way and not on the basis of biological necessity (law), and does not constitute part of the chain of laws governing evolution."[1]

How are we to understand these words? We want to be honestly persuaded that T. D. Lysenko is not compelling us to change the whole line of our understanding of Marxian dialectics. Here we have the denial of chance. We are taught the ABC of Marxism according to the works of the classics of Marxism-Leninism who justly teach us to regard chance as a form of the manifestation of the operation of law.

All these things create irreconcilable contradictions, mental confusion among the Soviet public; they are not to be settled by the type of speech we have heard here. They require deeper and more serious analysis, fraternal assistance to those who have gone astray.

What is causing me still greater concern? It is the fact that in his new works Comrade Lysenko is at variance not only with Darwin, Timiryazev and Michurin, but also with the basic principles of Marxism-Leninism, in the sense of his ability to read the concrete, clear and precise statements made by the classics of Marxism. In his address Comrade Lysenko appealed to a point in Engels' letter to Lavrov. Comrade Lysenko pretends to have discovered that in this letter Engels condemns both the fact and the theory of "overpopulation" and intraspecific competition in living nature. I have already attempted, in the *Liter-*

[1] *Ibid.*

aturnaya Gazeta, to correct this very grave error, which, in its essence, is a revision of the foundations of the Marxist doctrine, as being a relapse into Dühringism.

I assert that all the arguments that we have heard, and that we have seen till now in a number of the publications of creative Darwinists go to show that Comrade Lysenko and his adherents failed to understand Engels. In the quotation, which I shall not repeat here, Engels had in mind the errors committed by bourgeois natural-scientists and sociologists in employing the laws of the struggle for existence in nature, in applying them to human society in the spirit of Malthusianism. Comrade Lysenko and his adherents, I repeat, failed to advance a single new argument in support of their point of view, except those advanced in his day by Dühring, and refuted by Engels in his *Anti-Dühring.*

If this paragraph in Engels' letter to Lavrov evokes doubt as to the correctness of its interpretation, let us study not the form but the very essence of Engels' *Anti-Dühring.*

I have the book with me; let us reread the paragraph, for after all, repetition is the mother of learning. But what do we find in the *Literaturnaya Gazeta*?

"Engels considered the doctrine of intraspecific competition in nature to be so harmful that he considered it necessary to combat this doctrine."

"But it is quite evident that the doctrine of intraspecific competition which is supposed to be the inevitable result of a shortage of food for all the individuals generated, is the Malthusian doctrine, which has been refuted and discarded by the classics of Marxism-Leninism." (Avakian and others.)[1]

Now let me read what was written by Engels in his *Anti-Dühring,* a book he considered necessary to bring to the notice of the public at large, to the notice of world-wide public opinion and the working class, and not merely in a private letter addressed to Lavrov who, Engels believed, was literate enough to be able to understand his views:

"The main reproach levelled against Darwin is that he transferred the Malthusian population theory from economics into natural science, that he never got beyond the ideas of an animal

[1] *Литературная газета,* № 59, 1947 г.

breeder, and that in his theory of the struggle for existence he pursued unscientific semi-poetry, and that the whole of Darwinism, after deducting what had been borrowed from Lamarck, is a piece of brutality directed against humanity."[1]

Then after giving an exceptionally precise exposition of the historical content of Darwin's theory as a theory of selection based upon the individual characters of different beings, such characters determining the advantages possessed in the struggle for existence, by a given individual over other members of the same species, Engels in conclusion gives the following blunt reply to Herr Dühring:

"Now Darwin would not dream of saying that the *origin* of the idea of the struggle for existence is to be found in Malthus. He only says that his theory of the struggle for existence is the theory of Malthus applied to the animal and plant world as a whole."

And somewhat farther:

"And just as the law of wages has maintained its validity even after the Malthusian arguments on which Ricardo based it have long been exploded, so the struggle for existence can still take place in Nature, even without any Malthusian interpretation."[2]

What the classical writers of Marxism taught us was that while making use of the scientific achievements of the enemy we should subordinate them to the interests of the working class. Why, then, is it now desired to make us renounce Mendelism with the concrete facts it contains?

In Marx's works we see the direct confirmation and the highly positive estimation of the geometrical progression discovered by Darwin, or, what is the same thing, of the fact of overpopulation in the realm of plants and animals. More than that—Darwin refuted the Malthusian theory of overpopulation in human society by proving the existence of overpopulation in the animal and plant world.

What are Lysenko's adherents doing in order to defend his prestige even in those cases where the grossest errors against the Marxist doctrine have been committed?

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[1] F. Engels, *Herrn Eugen Dührings Umwälzung der Wissenschaft.* Moskau, 1946, S. 80.

[2] *Ibid.*, S. 82.

In the above-mentioned article in the *Literaturnaya Gazeta,* Comrade Avakian and others had recourse to the falsification of the ideas of K. Marx. While citing a brilliant passage from Marx's *Theory of Surplus Value,* they were prudent enough to drop two phrases preceding it. Here is the quotation in full:

"*Darwin* failed in his excellent work to see the fact that by discovering 'geometrical' progression in the animal and plant world, he was refuting the theory of Malthus. The Malthusian theory is based precisely on the point that he counterposes Wallace's geometrical progression of man to the chimerical 'arithmetical' progression of animals and plants. Darwin's book, for example, where the causes of species' extinction is discussed, contains, if one leaves aside his main principle, a detailed refutation, based on natural history, of the Malthusian theory."[1]

Thus, Marx directly corroborates and gives us a highly positive appraisal of the geometrical progression discovered by Darwin in the animal and plant worlds. Moreover, it is only when we take the full quotation and ponder over it that we can understand the truly dialectical construction of Marx's thesis. Darwin refuted the Malthusian theory of overpopulation in human society precisely by having proved the fact of overpopulation among animals and plants.

But what have Avakian and others done? They have taken only the last of Marx's three phrases for they understood that the first two phrases reduce to nothing the article they published in the *Literaturnaya Gazeta.*

Voice from the hall. But those two phrases were given in T. D. Lysenko's article.

B. M. Zavadovsky. I believe the contradictions arising in this sphere are of extreme gravity and responsibility. We must approach everything we have been taught by Marxism-Leninism and the accusations that it is desired to level against us, in all the honesty and conscience of Bolshevik scientists. If we do so, we should fully subordinate ourselves to what is right in T. D. Lysenko's work, and register what is wrong in it and what in my view contradicts the things I have been taught by the Party about the basic principles of Marxism-Leninism. And

[1] K. Marx, *Theorien über den Mehrwert,* Bd. II, I. Teil, Stuttgart 1921, S. 315.

I want, instead of being defamed for having honestly expressed my doubts, to be instructed as to how to reconcile Engels' *Anti-Dühring* with the new outlook of so-called "creative Darwinism." I did not find that in T. D. Lysenko's address.

It is our duty to place all the methods, forces and resources possessed by us, Soviet scientists and practical workers, at the service of the national economy in order to strengthen the economy of our country. But instead of pondering in the first place over the forms of uniting our forces, participants in the debate here spent too much effort on defaming and disparaging all those who hold different views. I believe the policy, the line of splitting, of introducing confusion into our ranks is a wrong one.

Academician P. P. Lobanov. The time allowed to Comrade Zavadovsky is exhausted. The majority present at this session are inclined to prolong it. There will be an interval of 7 minutes.

After the interval, B. M. Zavadovsky is called upon to continue.

B. M. Zavadovsky. In the first place I take the liberty of dwelling upon the way I understand the contradictions that are arising in the development of the Michurin trend.

From the very first days of my acquaintance with the works of T. D. Lysenko I welcomed and continue to this very day to place a positive estimate on several of his theses, namely, the theory of phasic development, and the summer planting of the potato in the South.

T. D. Lysenko's chief merit consists in the fact that he drew attention to the work of Michurin, which had been ignored by formal geneticists. I stress all this now as being a great service performed by T. D. Lysenko.

But if we wish to develop the Michurin doctrine, we should remember our duty to study the classics in the original. And here I must say that from this point of view T. D. Lysenko is tremendously one-sided in his work, where he develops only one aspect of the scientific heritage left by Michurin. Occasionally and for reasons I cannot understand, he to some extent defames the work of other of Michurin's followers, as for instance, that of N. V. Tsitsin, who is creatively and effectively developing these other aspects of the Michurin theory.

What constitutes the Michurin doctrine? That is what I

shall dwell upon now, and let somebody try to persuade me that I have a poor understanding of it.

The first and basic premise of the Michurin doctrine is the method of sexual, geographical, interspecific and intervarietal remote hybridization, as a method of creating an extensive variety of hybrid material which subsequently constitutes the object of breeding and selection in the way Michurin understands it. In his doctrine Michurin assigned first place to sexual hybridization, as being the prime basis of the material on which he worked, in applying the method of rearing and selection. In this regard Michurin is the continuator, the best continuator of the genuine Darwinian doctrine.

How is the problem posed by T. D. Lysenko? In his work *Heredity and Its Variability,* as well as in the address we have heard here, he actually reverses the succession and the degree of the significance of Michurin's principles. He says: "*Heredity* can be *destabilized* in the following way: 1) *by grafting,* i. e., by uniting the tissues of plants of different varieties; 2) *by subjecting plants to the influence* of the environment at definite moments when they are undergoing *developmental processes of one kind or another*; 3) *by crossbreeding,* particularly of forms sharply differing in habitat or origin."[1]

All the three points are formally correct. All the three points of Michurin's doctrine are included here, but their succession has been changed. First place is given to vegetative hybridization, i.e., to grafting, whereas Michurin's doctrine teaches us that sexual hybridization is the basis for creating forms of different kinds. With Michurin the method of grafting continues to be regarded as a method of asexual propagation, serving in the main to support the valuable forms selected by him. But as against the really formalistic ideas of his predecessors, Michurin introduced the proposition—a product of genius—that vegetative hybridization is not merely a method serving to fix the most valuable hereditary characters, but is also a method of further developing them. Therefore the method of grafting is not a primary but a secondary method, though a valuable one.

But, comrades, the Soviet public should be truthfully informed of the fact that the doctrine of Michurin is already as-

[1] Т. Д. Лысенко, *Работы в дни Великой Отечественной войны*, 1943 г., стр. 62.

suming new forms, and sexual hybridization, in T. D. Lysenko's system of views, is assigned to the third place. I have not yet found arguments and admissions sufficiently strong and convincing to explain why and for what purpose Trofim Denisovich is departing, in this regard, from the Michurin doctrine. But why should this not be stated here in plain language? I stand for innovations, and if anybody convinces me that the new form of the Michurin doctrine is justifying itself, I am ready to acknowledge it. But this should be stated in plain words, instead of being camouflaged, as V. N. Stoletov does, when he says that at bottom the Michurin doctrine came into being when T. D. Lysenko touched it. Why then are we to call it the Michurin doctrine? I think that is wrong, because it misleads the Soviet public, who highly value the theories of Darwin and Timiryazev, and believe that this doctrine of Michurin is a direct continuation of their doctrine.

The question arises, then, why are people disputing the brilliant works of N. V. Tsitsin who applies the other side of the Michurin doctrine, the method of sexual hybridization, works which are no less brilliant than the principles laid down in the works of T. D. Lysenko. The Soviet public should be given an honest answer, for, as they see it, both Tsitsin and Lysenko are continuators of the Michurin trend, whereas Lysenko and his pupils are trying to revile the Michurin method being successfully developed by Tsitsin. I think this stands in need of explanation, and I would like a businesslike explanation to be given here.

But, comrades, there are other points, too, about which I feel uneasy, because in other cases T. D. Lysenko comes into direct collision with the doctrine of Michurin not only in theoretical problems, but in problems of practice, too.

T. D. Lysenko resorts to such diffuse terms as "good" and "bad" agrotechnical conditions. By "good" treatment the author means heavy manuring, but this assertion of Lysenko cannot so easily be reconciled with the Michurin doctrine. According to Michurin it would seem that in some cases very fertile soil is of no benefit to the plant, and that too liberal a diet is harmful to young animals. The question as to what are "good" and what are "bad" conditions is not solved as simply as one might infer from the statements of T. D. Lysenko.

Here is one of numerous examples of how illogical T. D. Lysenko is. He advances brilliant innovatory ideas, but some of his points do not justify themselves; the system which he is trying to make the basis of Soviet biological science has not been adequately thought out.

T. D. Lysenko ought to have devoted more attention to these contradictions so as to ensure that everybody anxious to learn understands what it is all about. And if Michurin stands in need of correction this should be frankly stated. But when influence is exerted of a kind to hypnotize the masses with the name and authority of a highly-placed person, this does not guarantee the creation of Soviet science. That is what we are taught by our Party. But have we heard a single word of dissent, capable of correcting the present state of affairs and putting a stop to indiscriminate and unwholesome defamation?

Why was it necessary to class me as a Weismannist and formal geneticist? It was only done because I repeatedly pointed and shall continue to point to the errors in Comrade Lysenko's work, only because I repeatedly indicated that Comrade Lysenko, while being an innovator in one field, had become a considerable hindrance to many necessary and useful trends in other spheres. I have expressed this opinion repeatedly at sessions of this Academy, and in the presence of Comrade Lysenko; I do not conceal it. But am I for that reason to be defamed and to have labels attached to me?

Why have I been ranked among the Malthusians? Here is my pamphlet published during the war, entitled *The Racial Ravings of German Fascism,* the greater part of which is devoted to Malthusianism and social-Darwinism. Did those who have christened me a Malthusian know of this booklet? They did, for the simple reason that, while I was at Omsk, during the period of evacuation, I sent two articles to Comrade Mitin in the editorial board of the journal *Pod Znamenem Marxisma* in which I dealt with the same subject, and exposed Malthusianism and social-Darwinism. I received a reply that he could not publish my material because other articles were being printed. I have looked over these articles, and maintain that my material contained a number of points that were not dealt with in the other articles. It turns out that Comrade Mitin, who was then editor both of the periodical as a whole and of its

scientific and philosophical sections, is at a loss whether to believe I am a Malthusian, or to give credit to the things he has read about my works or learned from a number of my public statements, including the paper I read at the London Congress on the history of science and technique where I exposed Malthusianism.

I had to be defamed because I have to be turned into an obedient admirer of some man of talent. That sort of thing, comrades, is hardly needful or beneficial.

Now allow me to answer the notes I have received.

Question. Do you admit the inheritance of acquired characters? Give a specific answer.

B. M. Zavadovsky. In the diagram which I handed over to the presidium, my answer to this question is the following: As far as Lamarck was concerned all that existed was a scheme of the inheritance of acquired characters. Darwin was not sufficiently precise on this point; he yielded, admitting the inheritance of acquired characters, and thereby, in my opinion, committed a great error.

What has Timiryazev to say on this subject?

I. I. Prezent. Don't tell us about Timiryazev, let us know your point of view.

B. M. Zavadovsky. I am a disciple of Timiryazev and must say this. Timiryazev says that the problem of the inheritance of definite characters requires a differentiated answer. In particular, Timiryazev admitted the inheritance of definite characters in plant organisms, and considered it improbable in animal organisms.

I accept this formula of Timiryazev, as far as I understand it, in the following way. I believe that under definite specific conditions inheritance is possible. The problem is not settled by schemes of a kind that are or are not universally accepted. Where it is a matter of the simplest organisms, where the organism is, in a certain sense, protected against the influence of the environment, there are no grounds for denying the inheritance, under definite conditions, of definite characters, in the simplest vegetable organisms. In my book *Darwinism and Marxism* (1926) I cited a concrete general scheme of the inheritance of definite characters provided that influence is exerted by the environment through the agency of hormonal organs.

But I consider that this phenomenon is not so simple, and is neither so common nor so mechanistic as T. D. Lysenko maintains. I believe that in higher organisms, in particular in vertebrates, to the study of which I have devoted my life, directed alteration can also be effected through endocrine organs, through definite chemical agents, the antigene hormones.

Let us agree not to indulge in defamation, but to state specifically why and how this is to be realized; but a struggle on two fronts is necessary, and let us be told where is the mechanistic front, where is Menshevistic idealism, and where is the correct front. For the time being, however, I am justified in thinking that to the best of my strength and ability I am defending the Party's position as to the struggle for its general line in solving the problems of the Darwinian theory.

Academician P. P. Lobanov. The session is adjourned till this evening.

SEVENTH SITTING

Evening, August 4, 1948

Academician P. P. Lobanov. We shall continue the proceedings of our session. I call upon Comrade F. A. Dvoryankin.

F. A. Dvoryankin (Editorial staff of the magazine *Selektsiya i Semenovodstvo* [*Plant Breeding and Seed Cultivation*]). At this session we have heard complaints from Morganists. I have in mind J. A. Rapoport, Doctor of Biological Sciences, and Academician B. M. Zavadovsky. I too want to complain about the rudeness of the critics of the Michurin trend. When they argue, they are not particular about the language they use; they, however, do not like to hear in reply a straight and true characterization of their actions, but complain about indiscriminate condemnation, defamation, abuse, etc.

Therefore, notwithstanding the accusations of the Morganists about the falsification of the views of the classics of Marxism, and other "compliments" of that kind, I will try to express myself in language that will be least offensive to them and keep close to the subject in hand. If I happen to depart from this new line in polemics on biology, please put me right.

We have been called upon here to appreciate the rational kernel in the biological sciences, to take into account the "numerous progressive trends in the biological sciences," not to lose contact with them, not to cast them out, not to substitute a very restricted movement for them. And our critics were pleased to depict this restricted movement in their own way and to give it out as the movement initiated by Academician T. D. Lysenko.

First of all, I think that we are indeed divided by the different attitudes we take towards the heritage of the classics in

the biological sciences, towards what the anti-Michurinists call the world biological science. According to the content that they ascribe to this term it is, in essence, territorial and not theoretical. At every stage of history, the world science of biology is represented by leading progressive science. Today, the progressive science of biology is represented by the Michurin trend in Soviet, progressive biology. That is why the apprehensions expressed by the Morganists about our isolation from the world science of biology sound strange to me.

Is it not surprising that in our country, where such immense changes have taken place, where scientists regard dialectical materialism, the revolutionary character of which is unquestionable, as the basis of all sciences—is it not surprising that for many years there should appear here dreams and trends that express the hankering of the Mendelists after unity between our science and what they call world science? They are dreams about some kind of an international language which, it is claimed, will grow around the concept of the gene (I have in mind here what was written in M. M. Zavadovsky's well-known article). It is a striving to preserve orthodoxy in classical Darwinism, to keep to the faith at all cost.

Do we not all, including those who preach this sort of thing, remember what Engels said, namely, that apart from the process that goes from the simple to the complex, from the lower to the higher, dialectics recognizes nothing as being once and for all established, nothing as sacred and inviolable?

Everything evolves, everything undergoes transformation. We stand for the heritage of the classics in science and not for swallowing all the conclusions of bourgeois professors, even if they are representatives of classical biology.

Lenin taught us that, while utilizing all the treasures of knowledge created by developing science, not to believe one iota of the conclusions of bourgeois professors, because they are the conclusions of men who look at the world, at nature, with the eyes of men of bourgeois society.

Our attitude towards the heritage of the classics in biology (I think I am right) is that of transformative assimilation, and not simply swallowing, not simply eclectically choosing methods from different sources. At one time the Morganists said that scientists must, like bees, collect honey from all flowers. It is

well known, however, that bees are able to discriminate and do not collect honey from every flower; we must assimilate what is useful, transform it from the standpoint of Michurin science.

What do the Michurinists take from classical Darwinism? They take into account—as the classics of Marxism taught—that the chief thing in Darwinism is the theory of evolution. What do we reject in Darwinism? The conception of flat evolution, which is inseparable from Darwin's conclusions if we do not revise them from the standpoint of the Michurin trend, if we do not single out what is true and valuable in Darwinism, what confirms the theory of evolution, and free it from its false pendants and departures from materialism, including Malthusianism.

What do we deem correct in Lamarck's theory? The organism's interaction with environment, the hereditary transmission of characters that the organism acquires in the process of this interaction. Did Darwin accept this? Certainly he did. Everybody will be convinced of this by studying Darwin's works. Did Timiryazev accept this principle in his works? Certainly he did.

But we reject the wrong aspects of Lamarck's theory—the autogenetic process of self-perfection of the organism, which, it is claimed, is inherently characteristic of all living beings. Who is developing this aspect of Lamarck's theory? The very people who call us Lamarckians, but forget to say that it is they, the Mendelist-Morganists, who are developing this aspect of Lamarck's theory. This morning B. M. Zavadovsky mentioned the stand taken by Dühring and, in keeping with his "third line" tactics, ascribed it to us, tried now and again to establish kinship between the Michurinists and Kropotkin, Smuts and Dühring. But in vain did he disturb their ashes. Dühring's conception was based on his formula: "plastically forming schematization." By this Dühring meant that the organism absorbs the substances it extracts from its environment and assimilates them, but it does not itself undergo a change. This theory of Dühring's implies the internal self-perfection of organisms independently of environment, the development of inherited characters independently of environment.

Permit me, my dear Boris Mikhailovich Zavadovsky, to return to you Dühring and his theory—it is entirely yours.

As regards the rational kernel in the science that is developing under the conditions of bourgeois society, we must bear in mind that the rational kernel in the various discoveries by foreign and pre-socialist Russian science lies embedded in a stratum of idealism. Every time you want to extract this deeply buried rational kernel, you have to perform an anatomical operation on bourgeois theory. We cannot imagine any other way of extracting the rational kernel from the bourgeois theories. What B. M. Zavadovsky offers us today as the rational kernel of genetics, with all its amendments, is nothing more than Darlington's line in Mendelism-Morganism, i. e., a concession to the new facts in form but the preservation of the old substances in theory.

Two different trends, two camps, have long existed in science, although the choice of trend by this or that scientist is not determined by the existence of these trends. This choice is determined by the ideological training of the given scientist, by his closeness to or remoteness from practice, and by other circumstances that help to mould his mentality.

We cannot regard the Michurin trend simply as the development of classical Darwinism. Under no circumstances. The two are qualitatively different stages in the history of biology.

The Darwinist clue to the approach to nature is the simple, though very important, conclusion expressed in the following formula: nature provides successive variations, man utilizes them in definite directions that he needs.

The Michurin clue to the approach to practical work on organisms corresponds to a higher conception, to a different stage of development. Man not only utilizes the successive variations provided by nature, but must deliberately call forth successive variations in organisms, must fix and develop them in definite directions by training. Therefore, the Michurin theory is the starting point for the development of the new socialist science, which is free from the errors and limitations of classical Darwinism. After this, there is nothing left to say about Mendelism-Morganism, with all the interpretations of its followers who, on the basis of Weismannism, united after Darwin in order to bury Darwinism. They assert that the theory of natural selection has collapsed as mere speculation, that there is nothing to corroborate it, that natural selection cannot lead

to anything except to the selection of extreme variations already existing in populations.

Who asserted this, if not the Morganist-Mendelists and all their allies? Many of these supporters of the anti-Darwinists are today orthodox Darwinists.

In view of the new practice that is developing in socialist agriculture, and in socialist construction in general, it is not enough for us to adhere to the treasure-hunting philosophy that is inherent in formal genetics. We have no use for the treasure-hunting philosophy in plant and animal breeding, agronomics and zootechnics, or in the other biological sciences that serve agriculture. The philosophy of utilizing the riches of nature that characterizes classical Darwinism is inadequate for us. Our philosophy in biology is to transform nature for the benefit of mankind. It is based on dialectical materialism. Michurin's theory teaches us not only to utilize, but also to increase the riches of nature, to create new and more perfect varieties not yet known in nature. This, of course, can be done on the basis of the laws of nature.

Amidst the controversy between these two, sharply distinct trends a "third line in biology" has arisen. This name was given to it by B. M. Zavadovsky at the famous debate at the Moscow University, where....

J. A. Rapoport. Where you were afraid to speak.

F. A. Dvoryankin. I have never been afraid to speak anywhere; and you need not to be afraid to heckle me, I am not on the way to a sanatorium.

At this debate at the Moscow University, the unprincipled bloc of all the anti-Lysenkoists, and that means anti-Michurinists, united and played the first scene of the old Russian popular play "The Mice Who Belled the Cat." I did not want to take part in that game.

The godfather of this "third line" was no other than B. M. Zavadovsky, who said that Academician T. D. Lysenko and his supporters were wrong in thinking that there are only two trends in biology. There is a third trend, the orthodox trend, the most stalwart representative of which is I. I. Schmalhausen.

Tomorrow, this orthodox Darwinist will proclaim that Schmalhausen is long out of date and will recommend

still another trend to serve as a line of retreat for "classical genetics." But in every new trend of his he preserves the foundation of foundations which unites them all, from my former teacher A. R. Zhebrak to Academician B. M. Zavadovsky. He will firmly deny that the characters an organism acquires under the influence of its interaction with its environment are hereditary, in spite of the fact that this proposition is the fundamental law of evolution which Darwin accepted and Timiryazev supported. It was perfectly evident to them that environment changes organisms.

What are the tasks of the "third line in biology?"

Objectively, the task of the third line is to preserve the unity between our science and that other "world" science, the address of which the Mendelists always give somewhere abroad. It is high time to tell them: turn to the U.S.S.R.—the home of world science has long been here.

In conformity with this task of preserving unity with bourgeois science at all cost, B. M. Zavadovsky worked out the tactics of the orthodox Darwinists.

The Mendelists exhort us to be honest and monotonously assure us of their honesty. They keep on assuring us of their honesty so often that one cannot help thinking that they themselves are doubtful as to whether they can convince their hearers.

The first tactical move of all the Morganists, and of B. M. Zavadovsky in particular, is to "Mendelize" Michurin. They are constantly falsifying Michurin's theory. For example: Rapoport, in his speech, as much as said that Michurin warned against being taken up with the training of plants and strongly emphasized the importance of Mendel's laws. As a matter of fact, Michurin devoted most of his time to fruit and vegetable plants and found no room for the application of Mendel's "pea" laws. Yesterday, a speaker here quoted what Michurin said about "margarine scientists"; this morning, B. M. Zavadovsky gave us a demonstration of true bureaucracy in science. He accused T. D. Lysenko of having, in his address, quoted the three theses of Michurin's out of their proper order. Michurin, he said, gave first place to hybridization, second to selection, and only third to training. Lysenko, however, he said, put training in the first place.

One must indeed be hopelessly scholastic to think, as Zavadovsky does, that the order in which these problems is put is the most important. They depict Michurin like this: first he was a Grellist, that is to say, a Lamarckian of the purest water; then, after a vast number of his plants had perished, he went over to distant hybridization and by scooping combinations from the fountain of the gene, achieved real success. This is not true. On this point it will be useful for all of us to reread the works of Michurin. He followed a different trend. After becoming convinced that Grell's method of acclimatizing tender varieties on frost-hardy stock was useless, and after inspecting a vast number of orchards, he obtained facts, which he deciphered himself and revealed Grell's mistake. He found that some varieties imported from abroad stood up to the severest winter conditions, but they survived because their ancestors in their native habitat had already lived through such conditions.

A second variety that survived on frost-hardy stocks consisted of plants that happened to be grafted to a strong stock and yielded to its influence. Grell did not know that a variety whose heredity had stabilized and which has been propagated vegetatively for a long time does not easily yield to influence. This creates the possibility of developing varieties on a semi-wild stock without loss of the inherited properties of the variety. This also creates the possibility of altering young, unformed, hereditary hybrid seedlings under the influence of a stable cultivated mentor—stock or scion.

Michurin began to work with hybrid seedlings, and as the hybrids, which combined within themselves the properties of local and southern varieties, were able, under the local conditions, to deviate in the direction of the hardier but wilder, uncultivated parent, he took up distant hybridization so that the hybrids should not meet at the place they were being reared with conditions native for both their parents. Thanks to this, the formation of their characters were bent in the direction the breeder desired, so that he could play with dominance and recessiveness, constantly change, in accordance with conditions, the appearance first of some and then of other characters, while preserving a definite constancy in the properties of the variety that is being reared. Mendel's blind combinations were alien to Michurin.

B. M. Zavadovsky's second tactical move is to search for ideological mistakes on the part of T. D. Lysenko and his supporters with the view to separating Lysenko from Michurin, and Lysenko's supporters from Lysenko, so that it should be easier to drive them piecemeal out of the field of science which they had penetrated in spite of the efforts of the Morganists. One can understand this move. It explains the attempts to prove that Michurin accepted the theories of the Morganists. It explains their recognition of Lysenko's theory of phasic development, i.e., what is "recognized" by foreign biologists, and their rejection of all the logical deductions of the theory of phasic development.

The question is, if you, B. M. Zavadovsky, recognize the theory of phasic development, was it not an experimental proof and demonstration, the first in the history of science, of the way new varieties are fixed, of the way new, acquired characters are transmitted by heredity?

By his experiments in Gandzha, T. D. Lysenko proved that at a definite stage of development, a change in conditions alters the chemical composition and physical structure of plants and thereby alters the biological requirements of plants. You say that you recognize the theory of phasic development, but that you regard all the rest as Lamarckism.

Does not that show, my dear Boris Mikhailovich, that you simply do not yet understand what Lamarckism and what the theory of phasic development are?

The third characteristic feature of this third line is, to complain about being gagged, while they themselves resort to the most shameless gagging of all those who agree with T. D. Lysenko. They complain, for example, about harsh polemics, but recall the article "Under the Flag of Innovation" in the *Literaturnaya Gazeta.* We recognize the hand, and we recognize who is referred to "under the flag of innovation." We also heard this talk about pseudo innovation under the flag of innovation later. It was not your fault, Boris Mikhailovich, that your new article arrived too late and the editors did not publish it, as you said here.

Let us recall P. M. Zhukovsky's "Darwinism in a Crooked Mirror." We do not complain. We rather like to read articles like that; here they follow the ancient Russian custom and say:

"I am going against you." Here there is no standing on ceremony. But when the Michurinists resort to harsh polemics against their opponents, a howl is raised about gagging, sticking labels and rudeness.

What did Boris Mikhailovich say this morning? What epithets did he use? We will enumerate them: "monopoly position in science," "they act only to please Lysenko," "law abiding," "talents and admirers," "falsifiers," "distorters of the views of Marxism-Leninism," and lastly, something was said about somebody having forbidden the defence of the general line of the Party in biology. Who forbids you, who is gagging you? We say to you—let us join forces and open fire on the bourgeois biologists. All the time you have been able to criticize the Michurin trend, but we have not found, we have not read any sufficiently sharp and well-grounded articles of yours against the reactionary Morganists abroad.

Where is your ingenuity in formulating arguments against the reactionary biologists abroad, Professor Rapoport, and all you other Morganists? We neither hear nor see anything of that kind. Let us together open fire on them. Nobody is forbidding, and nobody can forbid, controversy in our science. Let us, within the limits of Michurin genetics, fight for the best way of mastering Michurin's teachings, let us fight for the best way of applying Michurin's teachings in practice. If we do that, we shall have plenty of grounds for constructive polemics.

But our orthodox Weismannists and classical geneticists turn the stream of their criticism against the fundamental proposition advanced by T. D. Lysenko (tomorrow, these orthodox Darwinists will turn into orthodox phytohormoneists—you indicated this new line today, Boris Mikhailovich). Our orthodox Weismannists are very skilful at directing their stream of criticism against Michurin science. It is enough for T. D. Lysenko, on the basis of phasic development, to propose the agricultural operation of vernalization, for Doctor Vasiliev to get up and argue that the theory was advanced by Klebs, and that vernalization is simply the wetting of seeds before sowing, an operation that was known as long ago as the time of Pliny the Elder at least.

This is the distinguishing feature of all the Morganists. No sooner did T. D. Lysenko propose the cluster sowing of kok-saghyz, which, thanks to the energy displayed by Academician I. D. Kolesnik, who is collaborating with the front-rank collective farmers and agronomists in the Kiev Region, produced splendid results, no sooner was this method announced, which increased the yield of kok-saghyz roots tenfold, than Doctor Sabinin, a very sarcastic comrade in science, informs a large audience of students at the Moscow University that cluster sowing had been known to him since the time of Tamerlane. Where have you been since the time of Tamerlane that you did not propose cluster sowing, considering that for fifteen years the methods of cultivating kok-saghyz were copied from the methods of cultivating sugar beets, which were useless for cultivating kok-saghyz?

So much for your stream of criticism.

Academician Zavadovsky complained that he was misled, that he had not been given the opportunity to prepare for Academician Lysenko's address, that he was not given the opportunity to read it beforehand. I only want to remind you how your celebrated "debate" at the Moscow University was organized, when several speeches were prepared beforehand with the object of "beating the old man up in a gang," although that did not come off. You prepared for the purpose of smashing the Michurin trend headed by T. D. Lysenko. As you indirectly admitted this morning, you have been preparing articles almost since the end of the last discussion, but for some reason they were not published in Soviet magazines. I do not know why your articles were not published, but I know that anti-Michurin articles were published in some magazines. The magazine *Sovietskaya Agronomia* (*Soviet Agronomics*), for example, published an article by Doctor A. I. Kuptsov entitled "The Form-Building Process in the Plant World" in which all the Morganists' inventions about Michurin and Lysenko were resuscitated. In the same magazine we read an article by A. R. Zhebrak, written in his own peculiar style, but on the same lines. I don't know what hindered you, but you made thorough preparations, and, of course, it was not your fault that, to your surprise, your article failed to appear in the last issue of *Voprosy Filosofii* (*Problems of Philosophy*).

B. M. Zavadovsky complained that he is wrongly being described as a Mendelist. He blames us for that. But, Academician Zavadovsky, look at the list of scientists whom I. I. Schmalhausen refers to as the representatives of the only correct orthodox science. That is how we learn who the Morganists are. We compared that list with the one Professor Dubinin quotes. Blame them. By your actions you have won a well-established place among them. Why do you think that it is indiscriminate and hasty?

In what way do the arguments of Academician B. M. Zavadovsky about the inheritance of acquired characters differ from those of the Morganists? Here is his "consistent Darwinism." I am reading from the diagram he submitted here by way of refutation: "inheritance of acquired characters is a fact" (Darwin). The second formula reads as follows: "Doubtful, and at all events bears doubtful forms"—this point of view is ascribed to Timiryazev. Here is the third formula: "the point of view current at the present time—inheritance of acquired characters —is doubtful, needs careful study and strict verification in each individual case ... does not play the leading role."

And B. M. Zavadovsky advances this third point of view in the name of dialectical materialism.

Academician Zavadovsky is wrong in thinking that the dialectical materialist conception was invented by him and A. R. Zhebrak, who also lays claim to it. But this is not Marxism, it is Weismannism.

Academician Zavadovsky asserts that he has been fighting formal genetics for a long time, that he is helping the broad Michurin movement, is protecting Michurin's teachings from being narrowed down by Academician Lysenko.

As a matter of fact, all the assistance B. M. Zavadovsky is rendering consists in hanging on to the arm of the Michurinists to prevent them from fighting the Weismannists and the Mendelists, in urging them not to lose the rational kernel, while at the same time he fully approves of and supports the other front that is fighting the Michurinists. All we can say about such "assistance" to the Michurin trend is: God save us from the Zavadovskys; the Rapoports we can handle ourselves. (*Applause.*)

Academician P. P. Lobanov. I call upon the Scientific Director of the Mordovian Plant-Breeding Station, Comrade N. I. Feiginson.

N. I. Feiginson. Although the question of the situation in biological science is being discussed at a session of the Lenin Academy of Agricultural Sciences, it is by no means of purely academic interest, but has a great significance for practice, for production. Our agriculture is being built on scientific principles. It is far from being a matter of indifference to our workers in agriculture, for our foremost collective farmers, agronomists, seed growers and plant breeders, for all our rank-and-file scientific workers, what scientific theory is taken as a basis in their work.

Many of the speakers at this session have said that Mendelism-Morganism is divorced from agricultural production, that it has no relation to practice. But this is not true, it is a profound error. All that can be said is that Mendelism-Morganism has no connection with the real achievements of practical agriculture—that is undeniable. But it is also undeniable that the Mendelian-Morganian doctrine has caused much harm to practical agriculture, and this must not be forgotten.

On the other hand, the representatives of the Weismann genetics who spoke at this session pleaded with us not to discard Mendelism-Morganism, because, allegedly, it has been very useful to practice, to agricultural production, and promises to be of still more use. We have been hearing such assertions for many a year. Many years already have our Mendelist-Morganists been trying to persuade the Soviet public that theirs is a highly effective science, that it is of assistance to practical activity.

I think it was Napoleon who said that if you are not right, stick to your point and in the end you will prove right. This unprincipled counsel has been fully adopted by our Mendelist-Morganists. They know that they are wrong, but they stick to their point, reckoning that in the end they will be believed.

Where, when and by what method, I ask, has Mendelism-Morganism brought any benefit to agricultural production?

One of the Morganists who spoke at this session, Comrade Rapoport, mentioned the Lisitsyn winter rye variety, which is supposed to have been produced on the basis of the Mendelian

theory. But when you undertake to say something, you should know what you're talking about. P. I. Lisitsyn did, indeed, produce a rye variety which has been introduced into practical farming, but what connection did Mendelism-Morganism have with the breeding of this rye? How was the Lisitsyn rye obtained? By the method of mass selection from local material. It was by this method, too, that N. V. Rudnitsky obtained the Vyatka rye variety at the end of last century, and the German landowner, Lochow, produced the Petkus rye in the eighties or nineties of the last century, that is, when nothing had been heard either of Mendel or of Morgan. But there was a period in the history of Lisitsyn's rye when attempts were made to apply Morganist principles to it, when strict selection was practised with a view to obtaining homogeneity of type, uniformity of the external features of the variety. As a consequence of this selection, the yield of Lisitsyn rye disastrously fell. It was only because this sort of selection was stopped in time that the variety has been saved for practical farming.

It is not infrequently said that crossing, hybridization, is an example of the work of breeders using Mendelian methods. The logic of this assertion, which we have heard made at this session, too, is very simple: Mendel worked with hybrids, hence everyone who engages in hybridization is a Mendelist. That is "ruthless" logic, if you like! By the same token, since Mendel was a monk, all monks are Mendelists. But what connection does Mendel's work have with real hybridization, by means of which breeders produce new varieties? Who does not know that the science of hybridization, that methods of hybridization effective for plant-breeding purposes, were worked out by I. V. Michurin and T. D. Lysenko, and not by the Mendelists? I shall cite an example from breeding practice.

Work on enforced crossings of spring wheats has been conducted for several years at the Mordovian State Plant-Breeding Station, with the purpose of producing a new variety. In this work, the methods recommended by the Mendelists are being applied: selection of the parental forms according to the characteristics desired to be obtained, enforced crossing of these forms in order to unite the characteristics, and search for finished varieties in the progeny. Through the hands of the wheat breeders have passed dozens of combinations of crosses,

and thousands of hybrid progeny; from them, promising specimens are sown on the experimental plots every year, but not one of them has gone beyond the preliminary variety tests.

Equally barren has been the decade of work on the breeding of a variety of pea by the Mendelian method of hybridization. Every year selections are made from the hybrid material, the selected strains are cultivated to the fourth or fifth generations, and are then discarded. Every year new crossings are effected, including "distant" crossings, and all in vain. To obtain a valuable variety by these methods, the work must be conducted on a very large scale, and even then success is a matter of chance, which by no means always falls to the luck of the breeder.

We at the Mordovian Plant-Breeding Station have for the past three years been working on hybrid varieties of winter rye obtained on the lines indicated by Academician T. D. Lysenko (by means of selection from varieties cross-pollinated in varietal tests). In the station tests in 1947, one of these varieties surpassed the district standard varieties in yield by 4-5 centners per hectare. In the variety trials this year, when the weather conditions were very unfavourable, the new varieties yielded 1.5-2 c. of grain per ha. more than the district standards. The new varieties of rye have now been sent for government variety trial and are being multiplied in the collective farms of the Mordovian Republic.

How is it possible to compare the effectual Michurin doctrine with Mendelism-Morganism, which is so feeble and moreover pernicious to plant-breeding practice?

We are now being told of a new era in Mendelism—the method of polyploidy. It is asserted that after treatment with colchicine gigantic plants with colossal yields are obtained. Reference is made, for example, to Sakharov's tetraploid buckwheat, which allegedly gives a yield one and a half times or twice as great as ordinary buckwheat, and has a larger grain. We at our station have had something to do with tetraploid buckwheat; we have grown it. It appears that the plant is indeed somewhat bigger than ordinary buckwheat. The seed is also rather larger. But this is not the most characteristic thing about this buckwheat. Tetraploid buckwheat has a low fecundity: it gives a small yield of grain per plant (is this not, in-

cidentally, the reason for the largeness of the grain?). Consequently, as a variety it is of no use for production purposes.

It should be said that not a single variety of any culture obtained by the method of polyploidy is being cultivated in practical farming. And this is, of course, the supreme touchstone of this method, of this "new era" in Morganism.

Academician B. M. Zavadovsky said in his speech that we must use every method in our work, in order to ensure the progress of our agriculture. Yes, in order to ensure decisive progress in our agriculture in a very short period, much work must be done, and by our scientists too, but certainly not by every method, but only by those methods which lead towards the goal. The Mendelian-Morganian methods are only capable of diverting our forces into fruitless work, and we shall not use them. We have the Michurinian methods, which have already justified themselves and which ensure rapid and genuine achievements in the work of our plant breeders.

Such is the situation in plant breeding. And what do we find in other branches of agricultural science?

Take, for example, such an important department of seed growing as the testing of varieties. True, Academician B. M. Zavadovsky may say: why pile everything in one heap, what connection has the testing of varieties with Mendelism-Morganism? A very direct connection. Formal genetics has a harmful influence on the quality of variety trial practice. Many of the shortcomings in the methodology and technique of variety testing and district standardization of varieties must be laid at the door of the Morganian-Mendelian genetics.

Those at this session who attended the excursion to Gorki-Leninskiye will have seen what is the effect of the customary methods of variety trial in ascertaining the value of such material, for instance, as branched wheat. With the customary methods of variety trial, this branched wheat is lost, and it does not enter anyone's head to utilize it even as material for selection. The whole trouble is that, influenced by the Morganian theories, the methodology of variety trial approaches a variety as something self-contained, divorced from its conditions of existence, its conditions of cultivation. This is the fundamental defect of the existing variety trial methodology. From this follow other defects, as, for example, neglect of the need to give

an assessment of the value of varieties for practical farming, which may lead to unpleasant surprises in agricultural production. Here is a graphic illustration.

The Kazan 5+6 variety of winter rye is warmly recommended by the State Cereal Variety Trial Commission for a number of regions, including the Mordovian Republic.

With only superficial acquaintance with the facts, this recommendation may seem correct. In the experiments on the variety trial plots in Mordovia, Kazan 5+6 rye has indeed for several years given a yield 2, 3 and even 4 c. per ha. greater than that of the variety formerly accepted as the district standard. But this variety has one fundamental defect: it has a pronounced tendency to lodge. On the variety testing plots this defect escaped the notice of the observers, but it materially diminishes the value of the variety for practical cultivation. In addition, Kazan 5+6 is little resistant to surplus moisture. This defect was also missed by the variety testers, since for the purpose of experimentation on variety plots, it is the rule to choose patches of flat ground, where surplus moisture is likely to be manifested only in very rare years.

That is why it happened that when the Kazan 5+6 variety was multiplied and distributed to collective farms, unfavourable opinions of it at once began to come in from agronomists and collective farmers. It therefore follows that a variety cannot be confidently introduced as a district standard until it has been extensively tested in practical cultivation. Academician T. D. Lysenko has time and again warned of this, but, blinded by the formal "logicalness" of their methods and their conformity with Morganist ideas, the variety testers have paid no heed to these warnings.

A critical revision is necessary of the theoretical foundations, technical methods and entire organization of variety testing, and this important branch of seed growing must be brought into correspondence with the Michurinist teachings.

Very grave harm has been done by the Mendelian-Morganian genetics in the field of production of variety seed. I would remind you, as one example, of the history of the method of spatial isolation in the case of a number of cross-pollinating field crops (rye, buckwheat, etc.), which was introduced thanks to the critical sophistries of the Mendelists, and which

on closer examination, was found to be not only unnecessary, but even harmful to variety seed growing. The history of spatial isolation has been dealt with at length in our press, and there is therefore no need to go into it here.

A few words must be said on the subject of the growing of hybrid cereal seed. The Mendelists, including some who spoke at this session, take credit for the utilization of hybrid maize seed growing. They even venture to assert that the spread of the use of hybrid maize seed in our agriculture was retarded owing to the adverse attitude towards it of the Michurinists. A grosser misrepresentation of the facts it would be hard to imagine.

It should first of all be borne in mind that the use of hybrid seed in the growing of seed of cultivated cereals has nothing in common with the Mendelian-Morganian theory (K. A. Timiryazev, incidentally, said as much in his writings). All that the Morganists proposed was certain intricate technical methods of obtaining such maize seeds (preliminary self-pollination and selection of self-pollinated strains), which were very difficult to apply on a mass scale. This, obviously, suits the interests of capitalist seed firms, since in capitalist countries the methods proposed by the Morganists are beyond the capacity of the ordinary farmer.

Academician T. D. Lysenko has for a number of years proposed the wide-scale testing and application of intervarietal hybridization in the seed growing of many field cultures. The Mendelist-Morganists object to these proposals and hamper their practical application. Here is a characteristic example. In 1947, under the influence of Academician Lysenko's proposals, our station included in its scientific plan the testing of hybrid seeds (obtained by unrestricted intervarietal cross-pollination), in particular, of winter and spring wheat. This plan was sent for expert opinion to the Institute of Grain Husbandry for the South-East U.S.S.R. The Mendelists among the scientific workers of the Institute were violently opposed to this work, considering intervarietal hybridization impermissible in seed growing.

Who was it, if not the Mendelists, that retarded, and are still retarding, the introduction of hybrid seed in practical agriculture? And this is quite understandable, for the chief thing

in plant breeding, in the eyes of the Mendelist-Morganists, is to obtain homogeneity of hereditary characters in the variety. No wonder they regard "homozygosity," that is, maximum heredity homogeneity, an achievement even in the case of cross-pollinating cultures, such as winter rye (you will find this stated in the manual, *Theoretical Principles of Plant Breeding*).

There you have the theoretical tenets of the Mendelist-Morganists, and there you have the practical consequences of these tenets! And yet they continue to assert that their "science" is capable of benefiting our practical agriculture!

It is only to the pernicious influence of the Morganian theories that one can attribute the fact that to this day absolutely insufficient attention is paid in the process of seed growing to improving the yield qualities of seed. Our seed quality inspectors, in their work of testing and roguing seed material, still to a considerable extent copy the methods practised in seed control abroad, which are determined by the interests of the seed merchants. Of course, we must strive for high purity and normal germinating qualities of seed, but this is not enough for our agriculture. Yet even the majority of our plant-breeding stations pay no attention to enhancing the yield qualities of seed material.

True, the reports of many plant-breeding stations contain data of comparative tests which would indicate that élites produced by them have a higher yield than the seed of the surrounding collective farms. However, these tests are conducted in such a way as often to lead merely to self-deception, and to create an incorrect impression of the actual qualities of the élite seed. As a rule, for these experiments seed is taken from one or two neighbouring collective farms and compared with the station's élite. But for the purpose of comparative tests, seed material should be taken from the collective farms which show the highest yield (of the given culture); it must be taken from Stakhanov fields. Only in this way can a genuine assessment be made of the quality of the élite seed issued by the station.

I shall give an illustration from the work of the Mordovian Plant-Breeding Station.

An oat breeder undertook an experiment to compare his reproductions of Pobeda oats with the first reproduction of this variety obtained in a neighbouring collective farm. In his ex-

periment, the material of the first-year seed plot gave a per hectare yield of 23.7 c., the second-year seed plot gave 23.9 c., the seed of the super-élite gave 23.6 c., and the élite 26.9 c., while the seed from the collective farm gave a per hectare yield of only 18.7 c. Everything seemed as it should be: the station's reproduction showed a yield ranging from 5 to 8 c. per ha. in excess of that of the collective-farm seed. But when we took seed of this same Pobeda variety from fifteen different collective farms in the republic, including farms which had obtained a high oat yield, and sowed it for comparative tests, it appeared that on the average the station's super-élite gave a yield of 22 c. per ha., while the collective-farm reproductions gave 21.5 c. per ha., that is, practically no difference at all. In other words, the work on producing an oat élite at the station proved to have been unsatisfactory.

Similar illustrations could be cited in the case of other crops.

Unfortunately, it has to be admitted that the influence of formal science still very strongly dominates the minds of many scientific workers and practical agriculturists. And this is quite comprehensible, since, from primary school to university, our youth are educated and trained in the spirit of this formal biological "science." In order to overcome the influence of this reactionary science—Mendelism-Morganism—it is not enough simply to say that it is no good. Organizational measures have to be taken. And the first thing is to see to it that our youth are taught and that they imbibe the Michurinian doctrine, and not the reactionary doctrine of Weismann-Morgan-Mendel.

Academician N. G. Belenky complained in his speech of a Morganist university lecturer who, in spite of the request of the director of his institute, refused to teach the Michurinian doctrine. But I think the lecturer was perfectly right; it is impossible to compel him to teach the Michurinian doctrine, just as it is impossible to compel N. G. Belenky, say, to teach the Mendelian genetics. It seems to me that if N. G. Belenky had to teach the Mendelian-Morganian genetics, he would tear it to shreds and tatters.

I do not mean to say by this that those who now sympathize with the Mendelian-Morganian doctrine cannot re-educate themselves. There are undoubtedly honest people among them who will throw off the deadweight of Mendelian-Morganian er-

rors and honestly come over to the Michurinian doctrine. But however that may be, teaching of the biological sciences must be radically reconstructed.

It is also necessary to stop the extravagant propaganda of Mendelism-Morganism which is conducted through the press. The fact is that all the biological journals of the Academy of Sciences of the U.S.S.R. are under the control of a handful of Weismannists.

They have also entrenched themselves in many of the publishing houses which put out biological literature. A lecture by V. Sakharov, "Ways of Creating New Plant Forms," in which the ideas of Mendelism-Morganism are propagated, was recently published in pamphlet form. In this booklet, V. Sakharov, asserting that "the material factors of individual hereditary characteristics can be located only in the cells, and nowhere else," stresses the specific role "of the chromosomes as the vehicles of hereditary traits." Hence this author concludes that evolution is practically in its entirety built upon multiple increases of chromosome sets (polyploidy). "One thing is certain," writes V. Sakharov, "and that is that polyploidy is apparently the only means by which the individual races could have arisen, literally speaking, instantaneously." Completely discarding the materialist kernel of the Darwinian theory, and ignoring the Michurinian doctrine, the Morganist V. Sakharov makes bold to assert that "the chromosome theory of heredity is perhaps the biggest achievement in biology in our times."

Perfectly preposterous is the following statement made by V. Sakharov in the same pamphlet: "It is precisely in the case of experimental polyploidy that we have for the first time a partial realization of the cherished dream of biologists of directive production of definite hereditary changes."

Could blind faith in the fictitious achievements of the Morganian genetics go any further? V. Sakharov, it appears, does not know (or pretends not to know) about summer planting of potatoes, which is a most graphic example of directed alterations in the heredity of plants on a mass scale. Sakharov discards the numerous instances obtained by Michurinists of changing hereditarily winter varieties into spring varieties and spring varieties into winter varieties; he discards the vast amount

of material on vegetative hybridization. Yet all this material incontrovertibly testifies that the Michurinists have really mastered many ways of directively altering the nature of plants, in contradistinction to the Morganists, who continue to assert that this is impossible.

It is time to put an end to the unrestrained advertising of reactionary theories in biology. It poisons the minds of many researchers and even of practical workers.

The Michurinian doctrine—that progressive doctrine in biology—will continue to advance with gathering speed, will disclose all its potentialities, elevate biological science to heights unprecedented in history, and will bring inestimable benefit to our agriculture. (*Applause.*)

Academician **P. P. Lobanov.** I call upon Director of the Dokuchayev Institute of Agriculture of the Central Black-Earth Belt, Comrade A. V. Krylov.

A. V. Krylov. Academician T. D. Lysenko, in his address, and comrades who spoke in the debate have convincingly demonstrated the immense value for practical farming of the results obtained by scientific workers and research institutes that consistently adhere to the position of progressive agrobiological science, in contradistinction to the school of formal geneticists, who have produced nothing of benefit to our socialist agriculture.

Unfortunately, there are still scientists, faculties and research institutions in our country which in all their many years of activity have produced nothing of value either for theory or for practice.

Having no practical achievements to their credit in the matter of producing valuable breeds of animals or varieties of agricultural plants, the Mendelist-Morganist theoreticians are trying to play up to practical plant breeders and to persuade them to declare that it was the laws of Mendel and Morgan that helped them to produce new varieties. There was a time when they succeeded in this strategem, because it was considered good form to declare that a variety was produced with the help of theory; and there was only one theory—Mendelism-Morganism. But now this no longer works. However, attempts to secure the support of old plant breeders are continuing. They

were repeated at this session by Academician B. M. Zavadovsky, who cited the work of Lisitsyn, Shekhurdin and Konstantinov as a proof of the practical efficacy of formal genetics. Let us go more closely into these assertions of Academician Zavadovsky. What are these plant breeders noted for? What are their achievements? Lisitsyn's most important achievement is Lisitsyn rye and his clovers. The best plant-breeding achievement of Academician Konstantinov is the hard spring wheat Melanopus 69, and that of Professor Shekhurdin the soft spring wheat Lutescens 62. I ask: by what methods were these varieties produced? Does Academician Zavadovsky know? Apparently, he does not, otherwise he would not have ventured to say that they were produced on the lines of Mendelism-Morganism. Comrade Feiginson, who spoke before me, has already told how the Lisitsyn rye was produced.

We are also aware of the methods by which the spring wheats Lutescens 62 and Melanopus 69 were produced. The authors of these varieties, Comrades Shekhurdin and Konstantinov, are present here and can themselves explain to Academician Zavadovsky how these varieties were obtained. They, as well as the Lisitsyn clover, were produced by selection from individuals well adapted to the local conditions. Here, too, the Mendel-Morgan shuffling was not needed. The attempts of the formal geneticists to cite the work of old plant breeders in support of their arguments will not go down here. The majority of the participants at this session of the Academy are well familiar with our best varieties of agricultural plants and animal breeds, as well as with the history of their production. The Mendelist-Morganists cannot name a single breed of animal or a single variety of agricultural plant of practical value which was produced on the basis of their theory.

This general statement may be also illustrated by examples from the work of our Institute. It is known that in the Kamennaya Steppe (Stony Steppe) a whole series of outstanding vegetable varieties have been produced which are widely cultivated in practical farming: Egypt beet, Guerande carrot, Boston cucumbers, etc. They are all products of mass selection and gradual, generation after generation, acclimatization of foreign sorts, in adaptation to the steppe climate, to the new conditions of field cultivation.

Academician Zavadovsky referred to polyploidy, which is said to have yielded valuable results. There are representatives of the State Cereal Variety Trial Commission present here. Let them say what varieties created by this method have been accepted by them as district standards and recommended for cultivation in collective farms and state farms.

So far, such varieties are not to be found in practical farming, no more than is the long-promised perennial wheat. Nor will they be as long as the methods of training founded on a correct conception of the interconnection of the organism with the factors of external environment are not applied to the hybrids obtained by the polyploidy method. Not only have the Mendelist-Morganists failed to produce new varieties and breeds; they have also proved helpless in the elaboration of methods of improving existing varieties and breeds. They have not set themselves such constructive tasks, save for the celebrated attempts to produce wingless moths and to change the colour of the eyes of flies, to make them more pleasing—blue instead of red.

They are captives of West-European and American mechanistic and idealistic conceptions in their understanding of living nature and agricultural production. The plant organisms and the soil and other factors of the external environment are regarded by them in isolation, unconnected with one another, as having no interaction, as something invariable, "eternal," that cannot be altered or refashioned by man.

Such an approach is alien to the finest representatives of Russian agronomical science—Dokuchayev, Kostychev, Timiryazev, Michurin, Williams, Lysenko. There runs as a crimson thread through all the works of these eminent transformers of nature certain absolutely definite and distinct ideas. Stressing that the plant is the central object of the efforts of agriculturists, these scientists direct all their scientific activity to studying the demands made by plants on the conditions of life, to studying how plant organisms react to the conditions of their environment.

Their creative thought and practical activity were concentrated on working out ways of further perfecting plant organisms, of actively remoulding the conditions of external environment in order to render the labour of the agriculturist more productive, and in order that our agriculture might be less

and less dependent upon the elemental forces of nature. This is the general line in biology and agronomy: it is the line pursued by the foremost scientists of our country, headed by Academician T. D. Lysenko.

The line in biology pursued by Academician Lysenko is the only correct one, because it is fruitful, it enriches practice, it brings immense benefit to our collective farms and state farms. We have ourselves seen how in the past fifteen years Academician Lysenko has made so many scientific discoveries and of such character, that any one of them may bring renown to the name of the scientist and win a firm place in agronomical literature.

And what have we heard here about Academician Lysenko from Comrade Zavadovsky? What was he driving at, what did he try to convince us of? As he conceives it, Academician Lysenko is a mechanist, who has to be fought just as much as the formal geneticists. This is where Academician Zavadovsky's third line has led him.

Academician Zavadovsky ranks himself among the orthodox Darwinists; it follows from the meaning of his speech that he is the man from whom all biologists, including Academician T. D. Lysenko, should learn.

Would not a little more modesty be in place? It is not Lysenko, but you, that have excessive pretentions to infallibility and a craving to be the foremost teacher of biologists and agronomists. But what grounds have you for this? What scientific discoveries does Soviet biology owe to you? What have you new to offer to the theory and practice of agriculture? No, it is not you, of course, who are our teacher, it is not you that Soviet agronomists and biologists will follow. We shall follow the correct path, the one explored by T. D. Lysenko.

In earlier statements and in his present address, Academician Lysenko quite correctly refers to Michurin and Williams as the founders of our progressive trend in agrobiological science. They worked in different fields; Academician Williams paid more attention to general problems of agriculture, Michurin to the problem of refashioning the nature of plants. But their teachings are organically connected, they originate from identical theoretical premises—from the methods of materialist dialectics. It is not fortuitous that he who objects to Michurin's

teachings does not share the ideas of Williams; that, as a rule, he who is opposed to Williams' teachings in the sphere of agriculture does not agree with the teachings of Michurin. And, contrariwise, the followers of Williams are Michurinists, and all Michurinists are adherents of Williams' teachings. And it must be plainly said that in order really to understand the Michurinian theory, and still more its latest stage, which is connected with the name of Academician Lysenko, it is not enough to have studied the works of Michurin; one must also have a clear grasp of Williams' scientific heritage. He furnished the answer to the question which is worrying Academician Zavadovsky and which seems to him contradictory, namely, against which background and for which conditions new varieties of agricultural plants should be developed.

We are not only perfecting plants; we are also transforming the soil in the direction of improving the conditions for plant growth.

The development of agriculture in the collective farms and state farms is proceeding along the lines of creating a background of high fertility, and it is precisely for this background, and in the conditions of this background, that new and highly productive varieties of agricultural plants are being created, and should be created.

I should like briefly to instance the work of the former Kamennaya Steppe Station, now the Dokuchayev Institute of Agriculture, to illustrate the potency and effectiveness of the progressive theories of Dokuchayev, Williams and Lysenko. To this I want to devote the second part of my speech.

In the organization of scientific research in the Kamennaya Steppe, right from the beginning, in the days of Dokuchayev, what was taken as the foundation was an all-round study of the natural-historical conditions of steppe agriculture in all their connections and manifestations, the method of all-round action on nature with a view to refashioning it in the direction needed by man. Williams, developing the ideas of Dokuchayev and Kostychev, formulated a law which actually prevails in nature —the law of the equivalency or irreplaceability of the factors of agricultural production.

On the basis of this law, he worked out the system of travopolye farming, a well-knit doctrine which completely accords

with the demands of socialist agriculture. Williams' travopolye system and the Michurin-Lysenko doctrine constituted that fruitful basis on which the numerically small body of workers of the Kamennaya Steppe Station achieved no mean successes in their work.

The majority of those present at this session know that the main problem tackled in the Kamennaya Steppe was the elaboration of a highly fertile and stable system of agriculture in the steppelands of the semi-arid southeast. It will also be known that this work, begun by Dokuchayev in 1892, was completed only in the past few years. This was because the program of research was for a fairly long time narrowed down and was not conducted on the basis of Dokuchayev's ideas. It should be remembered that even in the post-October period, right down to 1935, the ideas of formal genetics dominated among the leading workers of the station. All this seriously hampered the solution of the problem set by V. V. Dokuchayev. In spite of the existence of shelter belts, harvests in the Kamennaya Steppe were small and unstable. Seed growing was on a low level. Almost every year at that period the regional land authorities and the People's Commissariat of Agriculture made legitimate complaints to the station on account of its absolutely inadequate output of variety seeds and their poor breed qualities.

Scientific research at the station attained its maximum development in the last ten years, when its workers firmly adopted the theories of Williams and Lysenko. Under Williams' immediate direction, the travopolye system was introduced at the station. At the same time, guided by the recommendations of Academician Lysenko, the station expanded and reorganized its plant-breeding and seed-growing work. We tried to give effect to the recommendation of Williams contained in one of his letters to us: "It must be firmly understood," he wrote, "that without all-round and coordinated work in the domains of agronomy, breeding, and planting of shelter belts, it is no use hoping for a radical solution of the problem of increasing the yield of agricultural crops and of advancing all branches of agricultural production in general."

And so, in the Kamennaya Steppe, researches of a unique character were organized, with a view to working out a system of agriculture for the Black-Earth steppelands, in which the

ideas of Dokuchayev, Williams and Lysenko were organically combined.

Thanks to correct theoretical principles and a close combination of theory and practice, the effectiveness of our scientific and practical work was greatly enhanced. The Kamennaya Steppe, as it looks today, is of great scientific interest as an example of bold transformation of waterless and treeless steppe country. It no longer resembles steppeland. Along the borders of the fields grow broad belts of trees; ravines and dells have been turned into ponds and reservoirs. The steep banks of ravines and gullies have been planted with trees; copses have been planted near the gullies. In the areas between the shelter belts, field crop and grass rotations have been introduced, and each rotation has its own definite system of manuring and tillage.

Only select seed of varieties best adapted to the local conditions, varieties of our own production or of other stations improved by us, is sown. Under the influence of all-round travopolye farming, the fertility of the soil is being steadily enhanced, and its structure perfected. The micro-climate is improving and the water regime is becoming more and more stable, while the processes of water and wind erosion are disappearing. The result is that the yield of agricultural crops is growing and becoming more and more stable. Thus, by taking the doctrines of Dokuchayev, Williams and Lysenko as the basis of its work, our Institute has solved a major agronomic problem, has shown by concrete example—on a large land area—how by means of measures which are easily practicable by state farms and collective farms, natural conditions may be created for the obtaining of high and stable harvests in unfavourable, arid climatic conditions.

A number of examples could also be cited showing how, on the basis of the correct theories of Williams and Lysenko, important theoretical and practical results were obtained directly in the branches of plant breeding and seed growing.

Plant breeders and government variety experts know that in the past the Kamennaya Steppe Station did not produce a single outstanding cereal variety which was accepted as a district standard and widely distributed for practical farming, even though the breeding of grain varieties had been conducted there

for a considerable time and on a fairly large scale. What prevented this was that the breeders working on these cultures were under the sway of formal genetics.

But new people came, with new outlooks, and the situation changed. Comrade Vodkov and Comrade Nikolayeva have produced a high-yield variety of winter wheat, Stepnaya 135, which has already passed the government tests and has received very warm approval from practical farmers, not only in the Voronezh Region but in a whole number of regions of the Ukraine. This variety was produced in a new way. It is awnless, but was bred from the awned variety, Hostianum 237, without hybridization, by the method of selecting plants which had changed under the influence of new conditions of life and systematically training them against a background of travopolye farming.

This is not the only achievement of the winter cereals breeding sector. Broadly applying Michurin's teachings, it is working very fruitfully and has submitted for government trials several other new varieties of winter wheat.

A second example: many are familiar with the old Mup rye variety produced by the Kamennaya Steppe Station, which even in its birthplace, the Voronezh Region, was inferior to other district standards and was marked down by the State Cereal Variety Trial Commission for withdrawal from cultivation on the farms. But it was sufficient to apply to this variety a new method of improved selection recommended by Academician Lysenko, and to cultivate it against a background of travopolye farming, for a "miracle" to occur. In 1946, on all the variety trial plots of the Voronezh Region, except one, this variety took first place in respect to yield. After a detailed examination of the behaviour of this variety, the Voronezh government variety experts were convinced that in 1945 and in 1944, that is, ever since the variety trials of the new Mup élite were instituted, it also proved superior to the standard varieties of the region, the ryes of Lisitsyn and the Voronezh Agricultural Institute.

Our herbage breeding sector has likewise achieved important results by applying the new methods proposed by Academician Lysenko. It has produced a variety of yellow hybrid lucerne, Stepnaya 600, which in hay yield is superior to the most outstanding standard variety, Grimm-Zaikevich.

I should like to say a few words on the subject of seed growing. The variety department of the Ministry of Agriculture may confirm that the Kamennaya Steppe Station, now the Institute, has for the past eight years supplied its region with high-yield élites in excess of its requirements. Why is this? Because a highly fertile background has been created as a result of the introduction of travopolye farming, and because we began to produce élites by the method of Lysenko, employing intravarietal crossing and additional pollination and better selection, which we are continually improving.

For the past three years we have been studying the effectiveness of open intervarietal crossing. The results obtained are interesting. They fully conform with the data given by Comrade Olshansky. Open intervarietal crossing improves frost resistance, yield and quality of the grain of winter-wheat varieties. In the case of Hostianum 237, for example, in 1947, the ordinary seed gave a yield of 17.4 c. per ha., the second generation of cross-pollinated seed—18.7 c., and the third generation—20.8 c.

Particularly striking results were obtained by applying this method to foreign varieties which betray poor stability under our conditions. The per hectare yields in the case of the Clarken variety were: ordinary seed—4.2 c., cross-pollinated second generation—16.3 c., and cross-pollinated third generation—13.3 c.

We have verified the recommendations of Academician Lysenko in adaptation to the conditions of steppe agriculture. Comrade Dmitriyev has already told here of the success obtained in summer sowing of lucerne for seed in the Kamennaya Steppe; summer planting of potatoes, harrowing before ploughing and other methods have also proved highly effective.

It may thus be once more affirmed, on the basis of the fifty-six years' work of our research institute, that the successes and creative achievements were obtained in the period when its workers were guided by correct biological theory. And, on the contrary, the period when the scientific body was dominated by formal-geneticist ideas of living nature was characterized by stagnation and immobility.

The closer a research institution is connected with practice and makes it its aim to work for the benefit of the people, the more distinctly is the futility of formal genetics revealed, and

the clearer it becomes that the views of Michurin, Williams and Lysenko are the only correct views. With the names of these Soviet scientists is connected the creation of a new agrobiology, more progressive than the Darwinian theory. The more rapidly and profoundly agricultural scientists assimilate this progressive agrobiology and make it the basis of their researches, the more fruitful and creative their work will be.

In conclusion I should like once more to dwell on the question raised by Academician Zavadovsky concerning the right of the Mendelist-Morganists to widely propagate and freely expound their views. Academician Zavadovsky and other formal geneticists assert that Lysenko and his followers are gagging the mouths of the Mendelist-Morganists and are not allowing them to work. But this does not correspond with the truth. The very opposite is the case—it is the Mendelists who are making every effort, not only to keep in obscurity but to compromise as much as possible the Michurinian trend in biology. To the facts that have already been mentioned at this session, I might add the following. In 1939, I had occasion, at the request of the Committee on Higher Education, to investigate the way examinations were held in the Department of Genetics of the Timiryazev Agricultural Academy, headed by Anton Romanovich Zhebrak. And what struck the eye at once was that, among the questions put in the examinations to the students, there was not one on Michurinian genetics. The paper on which the examination questions were printed was so yellow with age as to look like Egyptian papyrus. I asked Anton Romanovich (incidentally, I raised this matter in the Council of the Timiryazev Academy), what was the explanation for this, why the questions were not freshened up, why there were no questions to test whether knowledge of the new, Michurinian school, the school of Academician Lysenko, had been correctly acquired. He declared that no new discoveries had occurred in science, and there was therefore no reason why he should put such questions to students.

This is the situation in the Timiryazev Academy to this day. The students know very little about the Michurinian trend in science. They are stuffed with formal-geneticist ideas in the realm of biology. It is only when they come to do practical work that they get some sort of re-education. True, for some

reason or other, they have stopped sending students from the Timiryazev Academy to our Institute.

Now, another example, this time from the Voronezh scene. Voronezh is a big higher education centre. It trains agronomists, zootechnicians and biologists. It has an agricultural institute, zoo-veterinary institute, and biological faculties at the university and the pedagogical institute. To whom has the Ministry of Higher Education entrusted the forming of the mental outlook of the students in the realm of biology? To the not unnotorious Mendelist-Morganist, Professor Petrov.

Petrov heads the department of genetics at the university, and, in combination with this, he heads the department of fruit and vegetable plant breeding at the agricultural institute. Petrov reads a course in genetics at the zoo-veterinary institute and at the pedagogical institute. He never misses a single opportunity at conferences or meetings, whether in season or out, to abuse in the most disgusting form the Michurinian trend and Academician Lysenko personally.

Furthermore, the university and the agricultural institute periodically call up "reinforcements" from Moscow—in the person of Professor Dubinin, who delivers lectures to the students, post-graduate students and scientific workers. These lectures are widely advertised, and a halo is created around the lecturer of being the leading scientist in genetics.

Petrov categorically forbids his students and post-graduate students to read Lysenko's books and persecutes everyone who in any form expresses approval of the Michurinian genetics. And, after all this, the Mendelists have the hardihood to declare that the Michurinists are gagging the Mendelist-Morganists!

No, unfortunately the fact of the matter is that many agricultural and biological schools suffer from a surfeit of Mendelist-Morganists. This situation must be rectified. After all, it is there that our agricultural specialists and biological specialists are formed.

It is absolutely essential that our students be taught correct theory—the Michurinian genetics. For they are the future builders of Communism who, at the behest of our Party and of Comrade Stalin, must further promote the advancement of agriculture for the benefit of our Socialist Country. (*Applause.*)

Academician P. P. Lobanov. I call upon Professor B. A. Rubin.

B. A. Rubin (Bach Institute of Biochemistry of the Academy of Sciences of the U.S.S.R.). One of the central problems in the theory of evolution is the interrelation of the organism with its environment. Academician T. D. Lysenko devoted much attention to this problem in his address, and he quite rightly stressed the immense importance of a correct, Michurin conception of the nature of these interrelations for agronomical scientists and for workers in all branches of biology. For, indeed, when trying to grasp the inner meaning of the chemical properties of an organism, the properties of the metabolic processes in the organism, the biochemist cannot ignore the influence which the changes of external conditions exercise on these processes. It is here that lies the key to an understanding of how the adaptive reaction of the organism to external influences is effected, a reaction which ensures normal development of the organism and, what is of most interest to the biochemist, the synthesis of definite organic substances.

Until now, the attention of biochemists working on this problem was concentrated on the so-called geographical, or climatic, variability of the chemical composition of plants, which furnishes an example of inconstant adaptation in ontogenesis.

The work, chiefly of Russian, Soviet scientists, has established many valuable and important facts relative to the dependence of the chemical composition of plants on conditions of the external environment. At the same time, these researches have not dealt with the nature of the influence exercised by the environment on the metabolic processes, with the causes which determine the specific character of the reaction of plants of different groups to external influences. The data obtained in these researches, highly valuable though they are, did not touch upon the important question of the inherent nature of the adaptive reactions of plants to environmental conditions. Proceeding from the teachings of A. N. Bach and A. I. Oparin on the biological role of enzymes in the plant organism, it is necessary when studying this problem to give prime attention,

not to ascertaining what particular chemical compounds are contained in the tissue of plants, but to investigate the processes which lead to the synthesis of these substances.

It must also be borne in mind that it is not only when the plant organism is transplanted from one geographical zone to another that it encounters altered external conditions. The plant encounters constantly changing conditions of existence all through its life cycle and in the course of every diurnal period.

The characteristic thing about these changes in plant organisms is that they occur in a regular succession, that they have a definite rhythm. Obviously, the plant organism can normally exist and develop only if the metabolic processes are suitably adapted to the constant change of external conditions. These are the basic ideas guided by which we, in the Bach Institute of Biochemistry, have been studying the biochemical nature of the adaptive reactions of plants.

In our researches, major attention is devoted to the leaf. I listened with the greatest attention to the speech of Academician P. N. Yakovlev, who, when telling of his experiments in training hybrid seedlings, stressed the immense influence which the leaf has on the properties of the forming organism, often determining the definite generic and specific character of the latter. Our researches over a period of many years fully corroborate this view. More, we have reason to believe that many also of the varietal characteristics of plants of one and the same species—such as early-maturation, storage qualities, yield (accumulation of reserve substances), etc.—may with a great degree of accuracy be characterized as biochemical properties relating to the leaf. In the case of many vegetable and fruit plants, this method may be effectively used by making a special selection for biochemical features.

The leaf is of special interest to the biochemist also because it is the organ in which are concentrated, not only diverse but even contradictory, antagonistic, functions. It is sufficient to recall that it is in the leaf that the primary act of formation of organic matter occurs, and, consequently, the leaf must be adapted to the processes of accumulation of assimilates. At the same time, this organ must be adapted to supplying the plastic substances of all the other parts of the plant. The concentra-

tion within one organ of functions so antagonistic in character permits us *a priori* to presume the existence within the leaf of a very refined, adjusted, and at the same time very dynamic system, under the influence of which alone the simultaneous performance of the functions of accumulation and translocation is possible. There can be no doubt that the functioning of this system is closely dependent upon the conditions of the organism's existence.

Our researches have completely confirmed these opinions. Particularly interesting is the data we have obtained pointing to regular rhythmic fluctuations in the activity of the leaf enzymes both in the course of the vegetative period and in the course of the diurnal period.

This problem was placed on an experimental footing for the first time by Academician A. N. Bach more than twenty-five years ago. A. N. Bach considered that the diurnal fluctuations of the activity of the enzymes in one and the same organism are connected with changes in its physiological state, and he emphasized the deep scientific interest of this problem.

In 1936, N. M. Sisakian established the existence of seasonal rhythms in the action of the enzymes which regulate the carbohydrate exchange in the leaves of the sugar beet. In 1937, we demonstrated that the activity of these enzymes does not remain constant throughout the twenty-four hour day, but experiences quite regular fluctuations.

Further researches elicited that the diurnal course of the enzymatic processes in the leaf are of very great importance, inasmuch as the intensification of the processes of synthesis in the daytime and of the processes of decomposition in the nighttime are suitably adapted to enable the leaf to perform the functions of assimilation and translocation.

These researches further showed that the diurnal rhythm of the action of the enzyme is not a direct reflection of the existing conditions, since it was preserved in plants which were placed in abnormal conditions (e. g., under light at nighttime, and in darkness in the daytime).

The diurnal rhythm of the action of enzymes is an example of phylogenetic adaptation, bearing the features of conservatism. But at the same time it was ascertained that the rhythm of enzymatic action cannot be regarded as an autonomous

property, since when the plant organism is subjected to abnormal conditions for a sufficiently long time, this rhythm is substantially disturbed.

Here we thus have a confirmation of Darwin's opinion that the rhythmic movements in plants are a hereditary feature, although, as he stressed, this periodicity may be disturbed by applying appropriate influences.

Our further work was concerned with a study of the internal mechanism of the adaptation of plants to the regular temperature changes taking place during the period of vegetation.

The study of this question was begun by V. E. Sokolova and myself in 1945. The chief object of observation was the potato, whose development, according to the work of Academician T. D. Lysenko, has an extremely close dependence on the temperature factor.

The processes of starch formation were chosen as the biochemical index, since they are most indicative of the specific directiveness of metabolism in the potato plant. The work consisted in studying the temperature curves of the processes of enzymatic formation and decomposition of starch in the leaves and tubers at various stages of the ontogenesis of the potato.

The chief conclusion to be drawn from these studies is that the temperature optima for the action of the enzymes are not constant. As the plant develops, these optima change, and the direction of the changes harmonizes very well with the course of the temperature changes in the plant's surrounding milieu. At first, roughly for two-thirds of the vegetative period, the temperature optimum for the formation of starch in potato leaves shifts towards higher temperatures, and in the last period towards lower temperatures. In the tubers, the starch-formation optima shift towards lower temperatures, apparently because the development of the tubers takes place in the latter half of the summer.

Consequently, the temperature optima for the action of one and the same enzymes in different organs of the plant are dissimilar, and their changes in the course of the vegetative period are likewise dissimilar.

In spite of the fact that the experiments were conducted

in years when the weather conditions varied considerably, the direction of the shifts of the temperature optima for enzyme action as a rule remained the same.

Hence the temperature curves of enzyme action, like the diurnal rhythm, cannot be regarded as a direct reflection of the conditions of existence of the plant organism. This characteristic was elaborated in the course of a long process of evolutionary adaptation, under the influence of that rhythm of temperature changes in which the plant's evolution proceeded.

These facts accord with Academician T. D. Lysenko's theory of phasic development of plants, according to which the demands made by a plant on its environment entirely depend upon its preceding evolutionary history, upon the environment in which the plant was formed.

The data we obtained also helped in the elucidation of the mechanism by which the mutual coordination of the functions of the various organs of the plant is attained. Experiments show that in the process of development of the potato plant there is not only a fluctuation of the temperature optima, but also changes in the temperature zones of starch synthesis and decomposition. Fluctuation of the optima towards higher temperatures is, as a rule, accompanied by a contraction of the zone of starch synthesis and a considerable expansion of the zone of starch decomposition. For example, in the beginning of July starch synthesis in the leaves took place already at a temperature of 15°, whereas at the end of August this process was to be observed only beginning with 28°.

The predominance at fairly high temperatures of the starch decomposition processes over the synthetic processes should assist the release by the leaves of a certain part of the assimilates accumulated in their tissues during the daytime. This would well explain the earlier observations of Chesnokov and Bazyrina, who discovered that in the first half of the vegetative period of the potato the curve of translocation of the assimilates from the leaves has a double peak, and that one of the maxima occurs in the earlier half of the day.

Widening of the zone of starch synthesis in the course of the development of the potato was also observed in experiments on tubers. For instance, in the tissues of young tubers

selected at the beginning of August, starch synthesis occurred only in the high temperature interval, outside of which the processes of starch decomposition predominated.

Hence, in the early period of development of the tubers, when the functions of growth predominate and the storage of starch is not the main process, starch formation is timed to occur in a very limited temperature interval, outside of which the processes of starch decomposition predominate. The biological significance of this regularity evidently consists in the fact that when soluble and easily mobilized forms of carbohydrate predominate in the tissues, the growth of these tissues proceeds more effectively. At a later period, when the growth of the tubers is less intense, and the storage of starch predominates, we observe a considerable expansion of the temperature zone of starch synthesis.

Further researches made it possible to establish that the optima for the action of the starch-forming enzymes in one and the same organ of the plant does not remain constant either in the vegetative or in the diurnal period.

It follows from the data we have obtained for tubers that the intensity of starch synthesis in their tissues is greater at nighttime, and, further, that this process coincides in time with lower temperatures, especially in the later stages of the potato's development.

A reverse picture was established in experiments on leaves, in which the processes of starch synthesis coincide with the higher temperatures. Further, both the general intensity of the process and its coincidence with the high temperature intervals leave no doubt that starch formation in the leaves, in contradistinction to the tubers, is a daytime process. It follows from this that the photoperiodic reactions of plants must under no circumstances be considered in divorcement from the thermoperiodic reactions, with which they seem to be most closely connected.

Our data likewise show that temperatures which favour the processes of starch synthesis in the tubers simultaneously stimulate the processes of starch decomposition in the leaves.

It is appropriate to point out that the so-clearly manifested coordination between the functions of the overground and underground organs of the plant is due to the regulating

action of the enzymes, which are specifically adapted to a definite state of the temperature factor.

Thus the periodicity observed in the biochemical activity of plants is a result of the development of the heredity of the organism under the influence of definite external conditions.

The data I have quoted is only part of the material at our disposal. We do not claim that they give an in any way full explanation of the problem under discussion. They only show that the specific peculiarities of metabolism are as closely connected with the conditions of existence of the organism as are the form and structure of the organism.

Academician T. D. Lysenko is constantly emphasizing that the forms of living bodies were created, and are created, solely by their conditions of life. Hence, changes of vegetable and animal forms can be directed only by skilfully directing the conditions of life of the plants and animals. A splendid example of such direction is the summer planting of potatoes suggested by T. D. Lysenko. Our data, in full accord with T. D. Lysenko's conclusions, also show that in this way the fullest correspondence is obtained between the temperature to which the potato is adapted at the various stages of its development and the actual temperature of its environment. The decisive factor in the degeneration of the potato observed in the South is the high temperature of the soil at nighttime, which far exceeds the temperature level to which the tuber-formation processes in the potato are adapted. Here we have manifested the dual nature of the potato, as a plant which, it is true, originated in the South, but in areas of high elevation. As we know, one of the distinguishing features of the latter is the wide amplitude between day and night temperatures, which is particularly characteristic of the last third of the vegetative period, when the tuber-formation process in the potato is most intense.

In full accordance with the theory of phasic development of plants, our data show that plant metabolism is adapted, not to a constant but to a successively changing temperature. The regular changes of temperature must embrace not only the entire life cycle of the plant organism, but also the separate diurnal periods.

We therefore see that metabolism in a plant not only reflects its definite biological peculiarities, but at the same time performs the important role of creating a unity between the organism and the conditions of its life, that dialectical unity which, as Academician T. D. Lysenko has rightly observed, lends a body the properties of animation and renders it fundamentally different from inanimate bodies. (*Applause.*)

Academician P. P. Lobanov. I call upon F. K. Teterev, head of the Department of Fruits of the All-Union Institute of Plant Industry.

F. K. Teterev. The highly illuminating address of Academician Trofim Denisovich Lysenko was illustrated by abundant documentary data. The achievements of Soviet agronomy and biological science are the most effective corroboration of the correctness of his views.

It was solely the Michurinian genetics which made these scientific achievements in our country possible. The All-Union Institute of Plant Industry, as you know, is engaged in work on plant resources—it collects them, studies them and distributes the finest plants for practical production.

Before the war, our work was conducted on the basis of morphology and cytogenetics. But since the war we have begun to study plant resources on an agrobiological basis. We have begun not only to conduct observations on the collections, but to study them with the view to their utilization for production purposes. Now, as a result, all sorts of accusations are hurled at us: it is said that the Institute of Plant Industry has changed, that it has ruined the collections, that it is not engaging in systematics, which is its real function. More, members of the Academy of Sciences have suggested the formation in Leningrad of an Institute of Applied Botany in place of the Institute of Plant Industry. Various facts obtained by the Institute of Plant Industry corroborate the effectiveness of the methods of agrobiological science. I shall give a few examples.

Everyone is familiar with the sour cherry variety, Krasa Severa, the hybrid obtained by Michurin by crossing the Winkler white sweet cherry with the sour Vladimirskaya. In the vicinity of Michurinsk and in a number of other regions,

this hybrid did not produce a sufficiently constant and abundant yield; it was declared to be a low-yielding sort, and on these grounds it was eliminated from the standard varieties of many regions. I. V. Michurin pointed out that in order to obtain a good crop from this variety, definite conditions were necessary. For example, with Mikhailov, at the Zubchaninovka Station, near Kuibyshev, Krasa Severa yielded, and still yields, big crops. But in the Leningrad Region, where this variety is not handled properly, its yield is poor. At our experimental base in Pavlovsk, in the Leningrad Region, Krasa Severa, when grafted on the Lotovaya sour cherry and pollinated with the Lotovaya, gives a very rich harvest. All the data on this subject has been communicated to the journal *Agrobiologia,* and all the data of the microscopic investigations are preserved in our Institute. Many of those present here had the opportunity when our Scientific Council held a visiting session at the Institute, to see what a fine crop this variety yields.

What is true of Krasa Severa applies in general to many other Michurin varieties.

Any good variety may be ruined if the proper conditions are not created for it.

Another example: Bellefleur-Kitaika, in our parts, where the conditions are not suitable for it, sometimes bears a poor crop; but around Krasnodar and in the Crimea, where the conditions are good, it bears a fine apple with a candy taste, similar to the apples which Michurin obtained from it.

The same may be said of many other varieties, even longstanding ones. If we create the necessary conditions for their growth and development, we undoubtedly obtain good results.

One of my jobs is to experiment in the intergeneric hybridization of stone fruits. In 1934-35, we obtained a hybrid by crossing the Vladimirskaya cherry with the Stepnoy almond. Quite by chance, as happens with the formal geneticists, the hybrid turned out to have 24 chromosomes, although a number of the seedlings were found to have another chromosome number. The haploid number of the almond is 9, that of the Vladimirskaya cherry is 16, yet the hybrid had 24 chromosomes. It blossomed profusely, but bore no fruit. But when this hybrid was grafted on to a sour cherry, it blossomed and began to bear fruit.

More, when this hybrid was grafted on to a sweet cherry, it even began to bear quite abundant fruit. In taste, it is a spoilt Vladimirskaya (to use Lysenko's expression), but that is not the point. The important thing is that this hybrid between a sour cherry and the Stepnoy almond normally bears fruit under the influence of mentors, although it has only 24 chromosomes, in other words, is an illegitimate hybrid, in the view of the formal geneticists.

Not only hybrids of sour cherry and almond and hybrids of bird cherry and sweet cherry, but other interspecific hybrids, which "according to the book" ought not to bear fruit, do bear fruit if only the necessary conditions are created for them.

As we know, there are many workers on distant hybridization; but, unfortunately, this work is often done so incorrectly as to be fruitless. B. M. Zavadovsky, for one, obviously has a muddled idea of the Michurinian doctrine. He said that the cornerstone of the Michurinian doctrine was hybridization. The fact of the matter is that hybridization was practised long before Michurin, but it was only Michurin who began to create varieties by alteration and training. Before crossing, Michurin properly prepared the initial components, providing proper conditions for them, approximating them by grafting, destabilizing their heredity, and so on. Has Academician Zavadovsky forgotten Michurin's methods of preliminary vegetative approximation, the use of "intermediaries" and the like? Michurin always began by choosing and preliminarily preparing the two components, then crossed them, and completed the process of creating the variety by training the hybrid progeny.

That is where the crux of the matter lies, and not in hybridization, for Michurin always said (and I should know, since I worked with him) that the fundamental thing was proper choosing and proper preparation of the components, and as to crossing, any student, any schoolboy could do that.

Not every one has a knowledge of the methods of training and reforming, and not every one knows how to utilize them properly. The methods of agrobiological science are much more complex and intricate than the sketchy methods of Morganism. The formal geneticists used to say that distant hybridization was of no use whatever, and that such hybrids were sterile; but now they say that simple crossing is quite

enough and you may get a distant hybrid by it. That is absolutely untrue.

One of the Morganists who spoke here mentioned the plum. I might give the following information on this score.

I had occasion to work with Dr. Rybin at the Maikop Station in 1929, when Darlington and Lawrence announced, with the usual Mendelist-Morganist style of approach, that if we cross the blackthorn with the cherry plum (*Prunus cerasifera*) we shall get the cultivated plum. Rybin went to the North Caucasus, where there is a great variety of blackthorn and cherry plum, and began to cross them, hoping to obtain the cultivated plum, although the plum is to be found in every orchard both here and in the South. Rybin's cherry plums produced their group of varieties, his blackthorns their group, and so forth. As the result of his labours, Rybin obtained sterile hybrids between the blackthorn and the cherry plum. Finally, in 1947, he announced in the Scientific Council that he had produced sterile, semi-fruitful, small-fruited and low-quality forms, but that, on the other hand, they were equal to the plum in chromosome number. And in 1948 Rybin planned to cross large-fruited forms of cherry plum and blackthorn, reckoning to obtain what he desired and then to proceed to breed it. But I think we, plant breeders, will cope with this task more effectively.

As to polyploid forms of flax and buckwheat, these Rybin himself discarded, as being quite useless. And so it is a little too early to talk of achievements in polyploidy.

J. A. Rapoport. But there are polyploids in nature.

F. K. Teterev. In 1928-31, Darlington and Lawrence declared that the sour cherry originated from the sweet cherry, as a result of a doubling of the chromosome number. But the areas of distribution of the sour cherries and the sweet cherries are 3,000 kilometres apart. How then could the sour cherry have been produced from the sweet? Lastly, there is a dwarf sour cherry growing in the Urals which has 34 chromosomes. Did it also come from the giant sweet cherry, which in all other characteristics and biological features sharply differs from the Urals sour cherry?

None of the attempts of Darlington, Lawrence and our collaborator, Viktorovsky, and of many others to double the num-

ber of chromosomes with the help of colchicine, acenaphthene, by rooting of leaves and the like have yielded positive results.

An almond tree growing in the grounds of our Institute on its own roots blossomed every year, being pollinated with the peach, but bore no fruit. But when this almond was grafted on to a sour cherry tree, and then pollinated with the peach, 32% of its blossoms gave fruits, and when pollinated with a mixture of these varieties 100% of the blossoms fruited. We demonstrated these results to the Scientific Council when it visited us in 1947 and 1948. I exhibited here photographs and fruits on the branch.

This shows that the heredity of the almond is destabilized under the influence of the mentor.

Lastly, cuttings of Houghton and Byely Plodorodny gooseberries, when grafted on to currant, begin to bear fruit two years earlier than the controls and blossom more profusely. This points to a very strong influence on the hereditary basis of the plant.

Utilizing Michurin's methods and materials, as well as the rich collection of the Institute of Plant Industry, we have produced a new variety of sweet cherry which, in the Leningrad Region, is not injured by frost and yields an abundant harvest between July 15 and September 15. Of the 200 trees we have, more than 100 are bearing fruit.

The sweet cherry is a southern plant. We are now in a position to grow this cherry in the Leningrad Region in large quantities.

Here, for instance, is a Leningrad late sweet cherry which matures at the end of August and the beginning of September, and the fruit of which may stay on the tree right up to the frosts. We have fifty trees of this cherry and have given a large number to state farms and collective farms in the Leningrad Region. I am not able to give a proper demonstration of our celebrated Zorka, which in sugar content is superior to the southern sweet cherry. Its fruits are already spoilt, but the size of the yield may be seen from this branch. (*Exhibits.*)

The trees of this sweet cherry grow exceedingly well in the Leningrad Region. Why is this? One would think that the southern sweet cherry could not possibly grow there. Here again, Comrade Rapoport would say that the point is not that

we have transformed it, but the way in which we approach the matter. However, attempts were made before to grow the sweet cherry in those parts, but, growing on their own roots, they freeze and do not give what is wanted. We took a sweet cherry and grafted it on to a sour cherry—that is, with the help of a mentor, the sour cherry, we transformed developmentally young forms of sweet cherry.

Under the influence of a potent mentor—the sour cherry—the sweet cherry assumes the form of a bush, it manages to complete its growth and its shoots to mature before the winter, and this lends it frost resistance.

Why do fruit trees freeze, and what determines their frost resistance? As we see it, complete maturation of the wood and normal preparation for winter in the given conditions determine the frost resistance of the trees.

So it is possible to grow the sweet cherry in the Leningrad Region on a sour cherry. Again it will be said that there is no active interaction, no influence here; but this is just as untrue as it is untrue or doubtful that the Morganian-Mendelian genetics may prove effective. We, by using Stepnaya sour cherry as the stock, have got the Zorka sweet cherry to ripen a week or a week and a half earlier. By grafting this sweet cherry on to a Vladimirskaya sour cherry—which is a biologically nearer form (Stepnaya is, biologically, a remote form from the sweet cherry), we, strange as it may seem, delayed the maturation of the sweet cherry by a week or a week and a half.

This year and last year we demonstrated to our Scientific Council and to visitors one and the same variety grafted on to different stocks and having different periods of maturation.

Many other facts of a similar nature might be cited, but there is hardly any need to.

Before Michurin, fruit growers worked blindly, relied upon chance and did not know where to begin. But now, applying Michurin's teachings, Soviet agrobiological science, directed by T. D. Lysenko, is in a position to solve any problem. It is all a matter of time. Soviet plant breeders, armed with an effectual theory, are remoulding the nature of plants in the direction we need. (*Applause.*)

Academician P. P. Lobanov. The session is adjourned until August 5, 11 a. m.

EIGHTH SITTING

Morning, August 5, 1948

Academician P. P. Lobanov. Comrades, a proposal has been sent up to the Presidium to close the list of speakers.

For your information: the number of speakers on the list is 72; of these, 38 have spoken.

What is your opinion?

A voice. I move that the list be closed.

Academician P. P. Lobanov. There is a motion that the list be closed. Is there no other motion? I will take a vote.

The motion is carried. Permit me now to proceed with the debate on the address.

I call upon Academician V. M. Yudin.

Academician V. M. Yudin. We Soviet zootechnicians are witnessing the wide introduction, first in our colleges and universities and then in experimental animal breeding, of Morganist-geneticist theories and methodics.

It has been stated here more than once that the Morganist-geneticist theories taught in our colleges and universities are by their very nature reactionary. These theories do not facilitate but hinder the practical and fruitful activities of those engaged in agriculture, as well as those engaged in stock-breeding.

Unfortunately, once a fallacious theory has been inculcated, it is not so easy to eradicate it.

At this session we heard Comrade Petrov, who called himself a geneticist animal breeder. Comrade Petrov told us that he had paid tribute to the times, and had at one time been a formal geneticist. He had even written on poultry breeding from the standpoint of formal genetics. Now Comrade Petrov

thinks that he has abandoned his old line and has gone over to the Darwinist position and to the corresponding methods in zootechnics.

Unfortunately, I cannot agree with Comrade Petrov's last assertion, because, as is evident from his speech, he still fails to understand how to work on breeds properly, and still less how to produce new breeds.

In his speech, how did he describe the work of M. F. Ivanov in producing that splendid breed of Askania Rambouillet sheep? According to what he said, it was all very simple. M. F. Ivanov imported some Merino rams from America and he got the "Askania Rambouillet." And, according to what Petrov said, Ivan Vladimirovich Michurin and our Soviet poultry breeders also worked that way.

Apart from the fact that statements of this kind contain a good dose of worshipping things foreign, his statement, and his interpretation of things, have nothing in common with the zootechnical interpretation of animal breeding, and is not in accord with the facts.

I should like to say for Petrov's information that neither for producing new breeds, nor even for current replenishments of stud stock, do zootechnicians ever "import" or choose animals haphazardly; they choose them very carefully, with full knowledge about the breed as a whole, and of the specific features of their own herd or flock; they choose the animals in accordance with the immediate breeding aim in view, and spend days, weeks and even months on this. This is exactly how the late Mikhail Fyodorovich Ivanov went about it.

I am saying this to illustrate the point that the fallacious Morganist-Mendelist theories, and the simplified ideas about the important, complicated and creative work of animal breeding taught in our colleges and universities, are not soon discarded even when a man wants to do so.

Hence the necessity, first of all, of drawing the first conclusion, namely, that our young people who are studying at our colleges and universities must be relieved of this indoctrination with scholastic Morganist-Mendelist concepts.

Speaking of Morganist-Mendelists, I cannot refrain from mentioning the disdain with which these people have always treated zootechnical practice, the systematic work conducted

by animal breeders, while at the same time often indulging in self-praise, and making all sorts of promises.

In this connection it will be useful to remind the Morganist-Mendelists of the very instructive exchange of opinion that took place at the All-Russian Conference on Cattle and Horse Breeding as far back as 1926.

At this conference, the distinguished zootechnician Pavel Nikolayevich Kuleshov, who had spent all his life studying breeds of animals, delivered a brilliant and perhaps his last address on "Breeds of Domesticated Animals in the Historical Succession of Their Evolution."

Classifying breeds of domesticated animals according to their historical significance, Pavel Nikolayevich, in that address, stated that the Merino and Karakul breeds of sheep belong to pre-historic domesticated breeds and are very important for the further production of breeds.

Further, in speaking of the formation of the Karakul breed, this great expert on animals said the following:

"I am of the opinion that this is one of the most ancient breeds which had taken thousands of years of stud work to produce.... Consequently, this great, colossal improvement is the result of selection that went on for many centuries. And now, in jest, I would invite the most distinguished zootechnicians and geneticists to produce these two breeds of Merinos and Karakuls again. I think that anybody who found a solution for this problem without utilizing the blood of a merino or a karakul would deserve not only the Nobel Prize, but also the immense Bakewell Prize. It goes without saying that if these breeds are utilized, the change may be very rapid; but if they are not utilized, I am convinced that the Nobel Prize will remain unawarded for many centuries." (*Report of All-Russian Conference on Cattle and Horse Breeding,* April 7-14, 1926, Moscow.)

You see how highly Pavel Nikolayevich appraised these breeds.

He perceived in them the labour of men spent on them in the course of centuries.

What was the attitude of Professor K. N. Koltsov, the head of the Morganist-Mendelists of that time, towards the creation of these breeds, to the stud work that had been spent on them in the course of centuries? This can be seen from the speech he delivered at that conference.

He said: "Pavel Nikolayevich expressed doubt as to whether genetics could in a short space of time create a new breed or repeat breeds that took thousands of years to produce under natural conditions. If we define breed as a population possessing a vast number of diverse characters, he may be right; it will be a different matter, however, if we set out to produce only those characters that are valuable for us.

"Several attempts at obtaining a synthesis of known old breeds by artificial means have been made in this country; thus, a synthesis of the comb and several other characters of Oryol chickens was obtained by crossing breeds which had nothing in common with the Oryol breed."

"B. A. Vasin, a worker at our station," continued Koltsov, "assures us that he has produced a 'Karakul sheep,' that is to say, he has found the solution which Pavel Nikolayevich says deserves the Nobel Prize. But we make no claims to that prize, because, if we interpret the problem as genetics interprets it, it is not a very important one." These were the concluding words of Professor K. N. Koltsov's speech.

Indeed, nobody has deserved the prize. Twenty-two years have passed since Professor Koltsov made those remarks and still there is no "synthetic Karakul." As their practical application has shown, the methods proposed by the Morganist-Mendelists have not only failed to serve as a practical means of producing a "synthetic Karakul" as Professor Koltsov and Vasin claimed, but have even proved unfit to preserve the qualities of the Karakul breed.

To prove the unsoundness and uselessness of the Mendelist-Morganist methodological premises for pedigree livestock breeding I will deal with only two experiments which they carried out. I refer to what they call "testing the males," and to their attitude towards the females in the flock and their evaluation in stockbreeding. I will take examples from the field with which I am most familiar, Karakul breeding, although the Morganist-Mendelists have employed similar methodological premises in other branches of stockbreeding.

A classical example of the unsoundness of the methods the Morganist-Mendelists employ in stockbreeding is "testing the males by their offspring."

Proceeding from the theory that the inherited characteris-

tics of animals have no relation to productivity and environmental conditions, they recommended that the largest possible number of rams be tested, for they were of the opinion that the discovery of a good ram was purely a matter of chance. To ensure the accuracy of the "test," every one of the tested rams had to be mated with the largest possible number of ewes.

Working to this method, at the time formal genetics flourished, nearly all the ewes in the Karakul farms in the vast spaces in Central Asia came under the "ram test." Many people took part in the work of classing the progeny that resulted from this. All the records of the quality of the progeny from all the state farms were sent to Moscow. Here, at a board presided over by Professor Vasin, a precise mathematical calculation was made of the quality of the progeny obtained from each ram. In conformity with this "precise" calculation, the rams were certified as "improvers" or "deteriorators," and these certificates, handed out by people in Moscow who had never seen the rams or the progeny, were sent to the state farms.

Under these circumstances, it is not surprising that rams certified as "improvers" were actually "deteriorators," and "deteriorators" were "improvers."

The result was that in 1936, when rams tested by progeny were needed for a pedigree stockbreeding farm that was being organized, it turned out that in the Uzbek Republic, the citadel of Karakul sheep breeding, only one such ram could be found, and that of poor quality.

The practical breeders in our state farms and collective farms discarded the excogitated Morganist-Mendelist methods of pedigree stockbreeding and, utilizing the methods worked out by the late Academician M. F. Ivanov, and also the experience of outstanding Karakul sheep breeders, worked out methods which ensure not only the preservation of the high qualities of the Karakul breed, but also a considerable improvement of them.

Realizing the importance of an animal's phenotypical characters and their connection with hereditary characters, this new method first of all provides for a careful selection of the rams that are to be tested by progeny; the rams are chosen with an eye to the specific features of the flock and the immediate tasks connected with it. Account is taken of their

origin and the productivity of the parents and, for the later period, their environmental and feeding conditions. To emphasize the importance of a careful selection of rams, the term "testing the rams" introduced by the Mendelist-Morganists was discarded and another term, "selection of rams and testing them by their progeny," was adopted instead.

Naturally, the next step was the suitable choice of parental pairs in order to prepare for the appearance of rams of suitable quality.

The results of this method on this as well as on other sectors of pedigree stockbreeding, were soon seen in the practical achievements in pedigree Karakul sheep breeding. Not only are we now in possession of rams tested by progeny, but in a number of state farms and collective farms valuable lines and families of Karakul sheep have been produced. The output of first-class lambskins is growing. At a number of state farms and collective farms, this year, the first-class lambskins amounted to 90% of the total. In the Turkmenian S.S.R., the figure was 95.6% at the Ravnina State Farm, and 94.0% at the Uch-Aji State Farm. In the Uzbek S.S.R., big results in this respect were achieved by the Kara-Kum Pedigree Farm, where the first-class lambskins accounted for 91.8% of the total, and 600 élite young rams were put aside for other state farms and collective farms.

At the recent Moscow congress of Karakul sheep breeders, many speakers strongly urged the necessity of a more comprehensive study of environment, in which feeding conditions play the leading role and, in this connection, the study of the individual stages—phases of development of the Karakul foetus and its wool covering in the uterine period with the object of securing a more directed breeding of Karakul sheep.

In this connection, the works of T. D. Lysenko served as our guiding line. A knowledge of them, and still wider application of his principles in the breeding of Karakul sheep, promise immense practical achievements.

The Morganist-Mendelists totally misunderstood the tasks connected with the breeding of a contingent of high-quality rams, but their fallacious methods led to still greater blunders in dealing with females.

Thus, in an article entitled "The Appraisal of the Hereditary Characters of Karakul Rams," B. A. Vasin wrote: *"The selection of a small number of rams, but of really the best* genotypes, *may, in the course of a few generations, completely transform the quality of the entire flock,* and the selection of the ewes will have almost no effect upon the rate of improvement. This will be understood if it is borne in mind that it is impossible to judge the genotype of the ewes by one or two of her lambs, and it is of no use attempting to appraise the ewe by whether it belongs to one or another line, because these lines do not yet exist in Karakul breeding."[1]

This is a denial not only of the possibility of selecting ewes according to their progeny, but also of selecting them according to productivity in general. In particular, it is a denial of the constructive and creative significance of suitable selection of parental pairs and thus nullifies all pedigree work with the flock. Instead of this, the work of qualitatively improving the flock and rearing pedigree stock is switched entirely to "selecting a small number" of rams, "but really of the best genotypes." They entirely forget that without careful selection of both rams and ewes and their suitable pairing, the production of even "a small number" of high-grade rams is impossible. A small number of rams will not enable the work of breeding pedigree stock to be organized. A large number is needed so as to be able, amidst the great diversity of feeding conditions, and also because of the reproductive, hereditary, wool constitution and age differences among the ewes, to make a suitable selection and pairing of the animals.

The fact that formal geneticists ignore pedigree breeding and selection of ewes is in complete harmony with their denial of the significance of the mother as the environment in the uterine period of development, and with their failure to understand the importance of taking into account the development of the foetus in that period and the influence of that period upon all the subsequent stages of the animal's development.

B. A. Vasin's assertion that it is impossible to judge ewes for selection according to one or two lambs was also absolutely groundless.

[1] *Проблемы животноводства*, № 2, 1934 г.

The accumulated pedigree records of the Kara-Kum Pedigree Farm enabled us to verify the effect of the selection of ewes according to the quality of progeny. An analysis of about 15,000 offspring obtained from pedigree ewes in the course of seven years showed that ewes which had élite and first-class progeny in the first two lambings, in all subsequent lambings accounted for 56.7% of the élite and first-class progeny, and for the whole of their breeding lives they accounted for 80.9%. Sheep which had no élite and first-class progeny in the first two lambings accounted for only 31.9% of these categories in all their subsequent lambings, and for the whole of their breeding lives they accounted for only 14.9%, compared with 80.9% for the first-mentioned grade of sheep, i.e., 66% less.

Thus, it is definitely proved that it is possible to judge ewes for selection by one or two lambings. In spite of what the formal geneticists say, the analysis of the material shows that if ewes are judged for selection even by one lambing, the effect of selection is seen, although it is less than if the selection is made on the basis of judgment by two lambings.

Selection of sheep according to quality of progeny enabled the Kara-Kum Pedigree Farm to build up a flock of 1,000 sheep of high producing capacity, 73% of whose progeny is of the élite and first-class grade. During all the thirteen years of pedigree breeding work, the Kara-Kum Pedigree Farm never had a single ram of such a high productivity. The average productivity of rams during the thirteen years had been about 45 to 50%.

The question naturally arises: how is this high productivity of ewes selected according to quality of progeny to be explained? By the fact that the mother exercises more influence upon the quality of the progeny than the father, which is in complete harmony with what T. D. Lysenko says about the superiority of the maternal over the paternal influence upon the quality of progeny. I will take the liberty of observing to Academician Zavadovsky that the arguments he advanced yesterday to prove that it is impossible to apply the principles of vegetative hybridization to animal breeding are to my mind groundless and were prompted by his narrow conception of this problem. Vegetative hybridization must not be reduced to the mere grafting of tissues. A broader view of vegetative

hybridization must be taken. Properly speaking, it is the influence of the soma upon future generations. In the case I have dealt with, we see that the soma of highly productive sheep affect the quality of their progeny.

The question is: have the formal geneticists changed their opinion about the possibility of selecting ewes according to their progeny? We will find an answer to this question in the book by Glembotsky, Deichman and Okulichev, published in 1947, *The Stock-Breeding of Fine-Wool Sheep*. In spite of the fact that data is given in the book showing the effectiveness of selecting sheep according to quality of progeny, the authors make the following statement: "Yearling ewes that have reached mating age can be judged only by their phenotype and origin, for they have not yet had any offspring. If we were to decide to regard this appraisal only as a tentative one to be verified later by an appraisal of the quality of their progeny, this, first of all, would encounter considerable technical difficulties, for it would entail the organization of a very laborious individual registration of the pedigrees not only of the élite, but also of the class section of the flock. The chief objection, however, one of principle, is that such an appraisal is not very reliable owing to the smallness of the progeny."

We see, therefore, that in spite of the fact that they themselves quote data showing the effectiveness of selecting sheep according to quality of progeny, the authors of this book cling to the false, scholastic theories of formal genetics and, flying in the face of facts, assert that "in principle, it is impossible to select ewes according to quality of progeny." In this case, too, life will ignore the formal geneticists; selection of animals according to quality of progeny is going on and will go on, for it is one of the methods—and in our opinion a very important and effective method—of still further improving the quality of our socialist stockbreeding industry.

In conclusion, I would urge upon our zootechnicians engaged in science, as well as those engaged in practical animal breeding, to familiarize themselves with the achievements of I. V. Michurin and T. D. Lysenko. It is also necessary to familiarize ourselves with the rich heritage left us by M. F. Ivanov, who may justly be called the Michurin in animal breeding, and more boldly generalize the immense experience accumu-

lated by his numerous pupils and followers in collective farms and state farms. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician P. P. Lukyanenko.

Academician P. P. Lukyanenko. The scientists and plant and animal breeders who have spoken from this rostrum have quoted innumerable facts proving the immense efficacy of Michurin methods in plant and animal breeding. Since the majority of these facts concern fruit and vegetable plants, I will, with your permission, quote several examples of breeding work on winter wheat at the Krasnodar Plant-Breeding Station.

Michurin's fundamental principles concerning the choosing of parental pairs for the crossing, training and selection of hybrids are extremely effective also when working on annuals, and on cereal plants in particular.

The wide application of the hybridization method in breeding winter wheat at the Krasnodar Station revealed the extreme effectiveness of Michurin's principle of crossing varieties of different geographical origin.

All the hybrid varieties of winter wheat produced at the Krasnodar Station, and now widely used in practical agriculture, were obtained precisely as a result of the application of the principle of selecting pairs for crossing. In particular, the high-yield, high-grade winter wheat known as Novo-Ukrainka 83, now assigned for three regions and already occupying a large area in practical agriculture, was obtained by crossing Ukrainka and Marquis, a Canadian variety of spring wheat.

The immense efficacy of the properly applied Michurin principle of choosing pairs in crossing can also be illustrated by the example of the new extra-early and high-yield winter wheat for the Kuban, the production of which we have been engaged in lately.

The problem of breeding such a variety of winter wheat for the Kuban is of extreme practical importance. The fact of the matter is that, under the conditions prevailing in the Kuban, the winter-wheat crop fairly often perishes already in the ear-filling period. The reason for this is not only rust,

or the lodging of the wheat, which often happenes here to a large extent, but also the unfavourable air temperature and humidity in this period. At this period (in June) there are torrential rains and high air temperature in the Kuban, the grain loses its high quality, and this causes a reduction of the crop by 50% and over.

The unfavourable influence of these meteorological factors upon the ear-filling process can be counteracted by causing the ears to fill at an earlier period, when the air temperature is not so high, and for this purpose it is necessary to produce a variety in which the ear-filling process finishes, in the main, in May.

The production of such extra-early varieties for the Kuban is important also for organizational and economic reasons.

Winter wheat forms a large part of the crop in the Kuban, and the cultivation of varieties of almost equal periods of vegetation imposes a great strain upon farmers in the harvesting season. This strain could be relieved considerably if varieties of winter wheat that ripened at different periods were cultivated. In the case of wheat, as with other crops, high yields are said to be incompatible with very early ripening. Nevertheless, careful study of the initial material, proper choice of the parental pairs for crossing, the use of such geographically distant varieties as Kanred-Fulcaster of American selection and a spring variety of Argentine selection, and also directed selection led to the production of extra-early, high-yield varieties of winter wheat. The first specimens of early wheat possessed certain unfavourable characters, for example, low winter-hardiness, etc.; but we have now succeeded in producing a variety from which these defects have been eliminated.

In this new variety, earing and ripening of grains commences 12 to 15 days earlier than in the ordinary, relatively early varieties—Novo-Ukrainka and Voroshilovka. Notwithstanding its extra-early maturation, this new variety has the highest yield. In this respect, under our conditions, it is far superior to all the other varieties. Its average yield under the variety test was over 42 centners per hectare. It exceeded the latest Novo-Ukrainka 83 standard by 12 centners. The new early variety is distinguished for its high plasticity, which ensures the highest yields against a high agrotechnical back-

ground; against a lower agrotechnical background its yield is higher than that of all other varieties.

One of the principal characters aimed at in the selection of winter wheat in the Kuban is resistance to rust. As we know, not so long ago, as a means of solving the problem of producing rust-resistant varieties, the geneticists proposed, as the most radical means, the production of amphidiploid varieties with the aid of colchicine. They do this by the colchicine treatment of hybrids obtained by crossing the given variety with Triticum Timofeyevi, a variety of wheat known for being most highly resistant to fungi diseases. Our observations of the amphidiploid varieties obtained in this way showed that these varieties soon lose their rust-resistant properties. Furthermore, the amphidiploid varieties possess such unfavourable characters as brittle spikes and difficult threshing, which in themselves preclude the use of these varieties in practical agriculture.

Proceeding from Academician Lysenko's theory of phasic development, we proposed that rust-resistant varieties be produced by means of intraspecific hybridization. This principle consists in crossing varieties which develop rust-resistant properties at different phases and ages. By this method we produced the valuable economic varieties Kubanskaya 131, 133 and others, distinguished for their high yield and high rust-resistant properties.

The alteration of the nature of existing varieties by training, by changing the conditions for passing through the vernalization stage, can serve as a new method of plant breeding, as our work in particular has shown. It is not only that this method enables us to transform winter varieties into spring varieties and vice versa, although this too is of great practical significance, for it helps us to obtain more winter-hardy varieties on the one hand, and, on the other, winter varieties of crops whose economic value would be enhanced if they became winter varieties; for example, in my opinion it would be of great practical advantage to have winter varieties of rice. In changing winter-hardiness, a number of other morphological and biological characters are changed, and as a result of this a diversity of forms appears which enables us to select in other directions. In particular, in our experiments in converting the winter wheat Voroshilovka into spring wheat and spring wheat

027 into winter wheat, segregation was observed in respect to a character that is very important for us, namely, rust resistance. In converting the winter wheat Voroshilovka into a spring variety, the formation of a considerable number (over 20%) of highly rust-resistant varieties was observed, whereas Voroshilovka wheat itself is very susceptible to rust. On the other hand, in converting variety 027, which is highly rust-resistant, into a winter variety, we observed that some of the plants showed a higher infection with rust. This vividly reveals the dependence of the development of a biological character, in this case rust resistance, upon the conditions under which the plant passes through the first phase of development. For practical purposes, it is important that there is a possibility to obtain new, highly rust-resistant varieties from varieties that are susceptible to rust without crossing; but for this purpose it is necessary to study thoroughly the conditions that facilitate the development of the rust-resistant property. At the present time the altered varieties of rust-resistant Voroshilovka wheat are undergoing the variety tests. Among these varieties there are interesting spring winter-hardy varieties which, when planted in the spring, show a higher yield than the best spring varieties, and when sown in the autumn, exceed the yield of the initial Voroshilovka variety. The best varieties of the altered Voroshilovka wheat, thus universally utilized, will, next year, be sent in for the state variety tests.

Concluding my brief remarks, I want to say that we plant breeders stand for the further development of the Michurin trend in biology, for we have not the slightest doubt that the development of the Michurin trend will provide us with even more effective methods, by the employment of which we will be able still more quickly and confidently to produce highly-valuable varieties required by our socialist agriculture. (*Applause.*)

Academician P. P. Lobanov. I call upon the assistant editor of *Pravda Ukraini,* Comrade A. V. Mikhalevich.

A. V. Mikhalevich. Soviet agrobiological science is developing in close contact with practical agriculture in the state farms and collective farms. To every Soviet scientist the behest of the great Lenin should be sacred: in our country, he said, science must not be a dead letter or a modish phrase, but must become

part of our flesh and blood, must, wholly and genuinely, become a constituent element of our daily life.

It is for this reason that I, a Soviet journalist, deem it appropriate at this session to draw attention to a number of facts characteristic of state and collective farming in the Ukraine, facts which may be of some weight in the scales of the present scientific disputes and controversies on the vital and urgent tasks of Soviet agrobiology.

Take, for example, a fact which cannot but rejoice all Soviet people—the fact that in the Ukraine a big harvest has been raised, and that in general the grain procurements are proceeding very satisfactorily. As of this day, twice as much grain has been procured in the Ukraine as on the same date last year. There are quite a number of districts which have already fulfilled this first commandment. And there are a number of districts which, although they had only recently been devastated by the enemy, have given more grain to the State than they used to give before the war. (*Applause.*)

There may be some who will say: all this is of course very good, but what connection has it with science, with the subject in dispute, with genetics? I admit that genetics, formal genetics, Mendelism-Morganism, whose barrenness may be regarded as demonstrated, does really have no connection with the advance fulfillment of the grain-procurement plan. (*Loud applause.*)

But there is another science—the progressive, Michurinian science, which willingly and voluntarily dedicates its achievements to the people, a science which is developing under the serene and approving eye of Comrade Stalin, the science associated with the names of Michurin, Williams and Lysenko. This science has unquestionably made its contribution, and is making an ever-increasing contribution, to the fight for grain, to the fulfillment of the grain-procurement plan ahead of schedule.

More than ten years ago, the shock workers of the collective farms gathered in the Kremlin to share their experience and to tell of what they further planned to do in the fight for collective-farm prosperity. At this memorable meeting, T. D. Lysenko told our leader and the foremost people from the collective farms of the first achievement attained by the fundamentally new, Soviet agrobiological science and the bold plans he cher-

ished for its advancement. Thousands of collective farmers remember J. V. Stalin's words of approval: "Bravo, Comrade Lysenko, bravo!" Since then, the Stakhanovite methods of numerous bumper crop growers in the collective farms have become still more closely interwoven with scientific creative effort, with the discoveries of scientists of the Michurinian trend, with their work on the improvement and remoulding of the nature of plants.

"But sometimes it is not well-known men of science who lay the new roads for science and technology, but men entirely unknown in the scientific world, plain, practical men, innovators in their field," I quote the words of Comrade Stalin, in his address at the reception given to higher educational workers in the Kremlin on May 17, 1938. This address has remained a book with seven seals to our Mendelist-Morganists, and to those who, like Academician Zavadovsky, would desire, in this full-dress discussion, to sit between two stools.

Comrade Stalin on that occasion cited examples of Stakhanovites who were upsetting the existing standards established by well-known scientists and technologists. "There you see what 'miracles' are still performed in science," said Comrade Stalin in reference to these examples.

When you carefully examine our living realities, you cannot help observing that in these past years these "miracles" in our science, in our life, have multiplied immeasurably. The roads laid in science and life by Stakhanovite practice, by the creative energies of the masses, are becoming ever broader and more promising.

Extremely noteworthy in this connection are the letters to Comrade Stalin published in our press communicating the obligations undertaken by individual republics, regions, districts, etc. Many of these letters contain the obligations of whole districts to achieve a sharp increase of yield of a number of crops on exceedingly large areas; for example, to increase the yield of wheat on thousands of hectares from 100 poods to 150 poods per hectare, and so forth. This is not only evidence of patriotism, of heroic labour; obligations of this order cannot be qualified otherwise than as bold scientific daring.

In the Ukraine, on the initiative of the Cherkassy District, which has been very greatly assisted by Academician I. D. Ko-

lesnik of the Lenin Agricultural Academy, dozens of districts are striving, in this one current year, to raise the average maize yield on the whole area sown to that crop to 40 centners per hectare. What this means is abruptly increasing the harvest by 20-25 c. per ha. This should give from two to three additional kilograms of grain per workday unit; it will create extremely favourable conditions for the advancement of pig breeding in the Ukraine, greatly increase the marketable surplus of the collective farms, and raise the standard of living of the collective farmers. Such an increase of yield would do honour to any scientific institution. But we should also bear in mind the gigantic scale of this "experiment" initiated by the collective farmers themselves.

But there is another, purely scientific, so to speak, side to the initiative of the Cherkassy District, which has the hearty support of the Central Committee of the Ukrainian Communist Party and of Comrade Khrushchov personally. Can it be denied that winning an average harvest of 40 centners of maize per hectare, in the varying conditions of soil, seed, implements, climate, agronomical methods, etc., prevailing in the collective farms, means solving at the same time certain highly interesting scientific, agrobiological problems? The foremost agronomists and masters of big harvests in many districts of the Ukraine are now enthusiastically engaged on this task, and there is every ground for believing that the aim they have set themselves will be achieved.

It would be useful for the elucidation of scientific truth, if Academician Schmalhausen were to spend some time this year in our Cherkassy District, and there, on the maize plantations, where a harvest of 40 centners per hectare will undoubtedly be obtained, talk to the people on the subject of whether the "reserves of variability" are decreasing—as he affirms in his book—or, on the contrary, increasing, and whether there is any sense in talking about "subsiding variety formation." (*Applause.*)

The profound process of increment of yield of a number of agricultural crops on large areas by means of better plant management, a process which has arisen in the course of mass competition for the fulfillment of the pledges given to Comrade Stalin, is worthy of the keenest attention of our scientific men.

The fundamentally important factor in this process is the organizing role of the Government, which, with its decrees establishing awards for high yields, gives a stimulus to heroic labour and scientific daring on the part of the broad mass of collective farmers.

It is in the light of this that an estimate should be given at this session of a positive experiment, the like of which is not to be met with in science before. I am referring to the assistance rendered by the Lenin Academy of Agricultural Sciences in increasing the yields of various crops on vast areas. I would recall to your minds the gigantic "experiment" with millet undertaken in the years 1939 and 1940, when the Academy, in response to the request of the Party and Government, rendered broad scientific agronomical assistance to the collective farms and state farms, and with them succeeded in securing an average millet yield of 15 c. per ha. on an aggregate area of 500,000 hectares, and of 20 c. per ha. on an area of 200,000 hectares.

V. R. Williams, in an article he wrote just before his death, laid especial stress on the profound and fundamental significance of this gigantic experiment with millet, as something new in the scientific activity of the Academy. Williams insisted upon the utmost extension of such concrete help by our scientific institutions to whole regions of our country.

Everyone in the Ukraine remembers the brilliant results of the assistance rendered by the Lenin Agricultural Academy to the Shpola District, where, in the drought year 1946, a splendid millet harvest, averaging 27 c. per ha., was gathered from the whole area planted to this crop in the district. Last year, 1947, the millet yield of 15 c. per ha. set as the target by the Government was gathered in the Ukraine, not from 300,000 hectares, as was envisaged in the assignment, but from an area two and a half times as great.

I might also mention the fundamentally important results obtained by the Kiev collective farmers with the help of T. D. Lysenko and I. D. Kolesnik in the growing of kok-saghyz (a leap in root-yield from 3-4 c. to 30-40 c. per ha. on the plantations of whole districts); and I might also mention the work now being done to sharply increase the buckwheat yield in the Dymer District.

In all this we may distinctly see such valuable qualities of the scientific trend developed by T. D. Lysenko as extraordinary efficacy of scientific work, its mass scale, Bolshevik energy in the practical application of scientific achievements, and a profound sense of responsibility for their proper application. And the people in the farms expect that these qualities will be still further developed.

One of the fundamentally important things which has to be stressed at this session is that, after Timiryazev, after Michurin, after Williams, you cannot do any useful work in biology if you discount the collective farmer and the active role he is playing in transforming nature.

Remember how eager Timiryazev was to keep in touch with the tiller of the soil; remember his hothouses at the Nizhni Novgorod exhibition. In the conditions that prevailed in tsarist days, he was unable to identify himself with the people. But Michurin we already picture surrounded not only by fruits, trees and plants, but also, and always, by collective farmers, horticulturists, teachers, young Communists and schoolchildren. And this is profoundly symbolical. When lauding the genius of Michurin at this session, there must be full appreciation of his profound and farsighted words about the new type of agriculturist, the collective farmer, who, he said, was destined to perform creative work of epochal importance. Michurin held that "every collective farmer is an experimenter, and an experimenter is a transformer of nature."

One of the decisive achievements of many scientists of the Michurin trend is their ability to understand the role and importance—the soul, I would say—of the new peasant, their ability to identify themselves with his creative audacity, to appeal to his dignity and experience.

It may be boldly asserted that, having passed through the school of Michurin agrobiology out in the fields, hundreds and hundreds of collective farmers in our Ukraine have, already after the war, grown into outstanding transformers of nature. I might instance the rubber growing experimenter, field team leader Batyushinsky, the celebrated millet grower Okhrim Zemlyanoi, the Heroes of Socialist Labour Elena Khobta, Polovkov and many others.

You comrades, the formal geneticists, do not even suspect

how these people have grown, what great strides they have made. They have their firm opinion on many questions which seem to you only subjects of academic controversy. No one can now succeed in robbing them of this new outlook on nature, no one can strike from their hands the weapon which makes them transformers of nature. (*Loud applause.*)

We know that the travopolye system, which is the pride of our native science, had numerous enemies, chiefly among those scientists who obsequiously worship everything foreign. But I shall read to you what a tractor driver of the Sitkovtsy District, Dmitri Palchenko, has to say on the question of travopolye farming. In a letter to our newspaper, he speaks in detail of his impressions of V. R. Williams' book, which, incidentally, in this district alone has quite recently been acquired by over a hundred tractor drivers.

"Every time I read this book I felt as if a bandage was being removed from my eyes. When I began to employ the method of mulching, and then ploughing with skim coulters, it seemed to me as if V. R. Williams had lit up in my mind powerful headlights of knowledge, which gave me the ability to see into the bowels of the land I was cultivating—that great storehouse of big harvests.

"I clearly came to realize that structureless soil, such as we find in many of the collective farms, acts as a brake on our progress. But who remakes the soil, if not we, tractor drivers, educated by the Soviet Government, the Party and Comrade Stalin?... And as I now understand it, the soil is cultivated not only by tractors and agricultural implements, but also by the roots of perennial grass mixtures. Without grasses, the tractor cannot have the power it may have when cultivating fields where lea rotation is practised. That is why I often look with pleasure and admiration at the seeds of our perennial grasses, especially timothy, of which in our Yatsenko Collective Farm we already have 26 hectares. The future prosperity of our collective farm depends on this...."

Here is another characteristic conclusion tractor driver Palchenko arrives at:

"Now that I know what grasses, mulching, the use of skim coulters, etc., mean, I cannot remain indifferent to the way leys are cultivated in the collective farms, or to whether they

send me to plough with a skim coulter or not. If they send me to a collective farm without skim coulters, I will buy them out of my own pocket, but I will not plough without them." (*Loud and prolonged applause.*)

Just try, if you can, to swing this tractor driver round to the position of bourgeois agrochemistry! No, he has already put his weighty tractor driver's hand into the scales of the old scientific controversy over the travopolye system of farming, and his word is decisive. Perhaps this is not yet realized even by certain travopolye scientists in Moscow. It is greatly to be wished that the "headlights of Williams' science," to use Palchenko's phrase, light up more brightly in their minds (*laughter, applause*), and that they acquire a more practical and progressive spirit.

Comrades, in these few days of our session there have been many appeals for truthfulness. Soviet journalists, educated by *Pravda* (*The Truth*), are accustomed to take this sacred word, truth, very seriously. It is not a word to be played with, as we have all seen adherents of Mendelism-Morganism trying to do here. The truth is profounder and more inexorable than many of them probably realize.

The truth is that it is not a few Michurinists whom the formal geneticists do not like, but our people, our absolutely new working class, our absolutely new Soviet peasantry, our absolutely new intelligentsia that resolutely purify science of everything antiquated, everything that is against the interests of the people, everything that is born of servile worship of the bourgeois West, everything that bears the stamp of idealism, everything that fetters the creative forces of the people and hampers their advance.

The truth is that not only within the walls of the Academy of Agricultural Sciences, within the walls of institutes, but in actual life itself, in the practice of millions, that the Michurin trend in biology has been weighed and tested and confirmed, and has earned the esteem of the masses. (*Prolonged applause.*)

The truth is that the most outstanding, profound and passionate adherent of the Michurin trend, T. D. Lysenko, has also been tested and recognized by the people, tested and appreciated in practical action—before the war, when by his work and his discoveries he helped the country to prepare for active defence, during the war, when the Michurin science helped to increase

the food resources of the country, and now after the war, when T. D. Lysenko is organizing the forces of agricultural science to take a most active part in the fight for communist abundance.

Some comrades who are divorced from life may perhaps find this hard to understand; but ask any Ukrainian farmer who, together with Trofim Denisovich Lysenko, and on his advice, vernalized grain; ask the Kherson or Odessa farmer who has seen what an extra yield the topping of cotton plants gives; ask any Ukrainian collective farm woman, who remembers very well how, in time of difficulty, Lysenko helped to save the sugar beet fields from a terrible visitant—the weevil pest; ask the Pereyaslav-Khmelnitsky or Cherkassy rubber growers, who, with Lysenko's help, have achieved real wonders on the kok-saghyz plantations; ask the collective farmers of the southern steppes, where Lysenko's science has helped to put a stop to the degeneration of the potato; ask the family of a Urals railwayman whose food situation, together with that of hundreds of thousands of other families, Lysenko improved during the war with his suggestion to use the rose end of potatoes for planting purposes; ask the soldier who ate the millet, of which the country was able to lay in substantial stocks thanks to the jump in crop yield obtained in 1939/40 as a result of Lysenko's work—ask all these ordinary people, and they will help you to understand wherein lies the virtue of the Michurin science, the virtue of the President of the Academy of Agricultural Sciences.

The truth is that, just as in Maxim Gorky our working class saw itself, raised to the peaks of culture, so in T. D. Lysenko our collective-farm millions see themselves, their creative urge to refashion nature, the personification of their talents in the struggle for communist abundance. (*Prolonged applause.*)

The truth is that Lysenko has long not been alone, that, with the help of the Party, a whole galaxy of Michurinian scientists have risen up around him. These are all men of action; we have heard them at this session, we know their work; they are not bookworms, they are men and women who are conscious of their duty to the people. And I think the truth is that all Soviet people are grateful to Comrade Stalin, to the Party and the Government, for having boldly replenished the Academy with new academicians of the Michurinian trend. (*Applause.*)

The truth is that the banner of Michurinian science widely

unfurled at this session opens up immense prospects for the state farms and collective farms, and will help to get the five-year plan fulfilled ahead of schedule. Many urgent practical questions arise in this connection. In particular, it is necessary not only to eliminate the shortcomings in the training of new forces, young specialists, but also to find ways of putting a speedy end to the shortcomings in the education of already practising agronomists and zootechnicians, so that every one of them may master the Michurin style of work, rid himself of the deadweight of false ideas, of formalism in his work, and become a convinced and militant exponent and practical organizer of the application to production of the science of Michurin-Williams-Lysenko.

The truth, lastly, is that the triumph of the progressive trend in biology at this session will have its influence upon the entire scientific front, which is so solicitously cherished by Comrade Stalin, will assist the entire front of science to perform its honourable duty in the fight for the building of Communism. (*Prolonged applause.*)

Academician P. P. Lobanov. Docent S. I. Alikhanian has the floor.

S. I. Alikhanian (Genetics Department of the Moscow State University). Comrades, it is very difficult for me, a simple lecturer at the Moscow University, to take the floor after such a fiery speech of the journalist who preceded me, but I shall try my best to lay forth my views on the scientific problems I have been investigating for the past eighteen years.

T. D. Lysenko in his comprehensive survey raised some vitally important problems in the field of contemporary biology. As it is impossible to cover these questions in a short speech, I shall merely express my personal views on heredity and variability, to the study of which I have devoted my life.

One of the principal postulates in Trofim Denisovich's address was the criticism of Weismannism. I shall proceed from this.

For the past fifty years genetics as a science accumulated a vast amount of experimental data. In my last year's report at the University I stated: "The course of its development, however, was not smooth; idealists of every kind and creed made attempts to utilize up-to-date genetical data as a reinforcement

for their idealistic theories and pseudoscientific, reactionary conclusions. I am referring to the metaphysical, idealistic conceptions of Johannsen, Weismann, de Vries, Bateson, Lotsy and others."

As this excerpt shows, I have never adhered to Weismann or to what is called Weismannism.

The entire course of development of our science has completely shattered all the idealistic theories of Weismann, Johannsen and others.

I shall take the liberty to remind you of some of the statements made by these scientists. Johannsen wrote: "Here we approach an extremely dangerous conception of the gene as of a material, morphologically distinct structure, a conception which impedes the sober study of heredity and against which we must utter an energetic warning."[1]

Bateson's theory which A. S. Serebrovsky so unhappily attempted to resuscitate is not less idealistic. From time to time this in its essence mechanistic theory is dragged out of the dust of the attic to characterize the data of genetics. Heribert-Nilson's theory is no less fallacious. The present-day experimental conception of the gene has nothing in common with these idealistic and metaphysical theories.

At the same time, I must point to the fallacies, mistaken conceptions of Serebrovsky, Filipchenko, Koltsov and others. Let us take for example Serebrovsky's postulate that the gene is the foundation of life. This he set forth to counter the mechanistic premise that life is a sum-total of physico-chemical elements. These extreme and absolutely unfounded conclusions were justly criticized. In my opinion such a conception of the gene is indeed extreme and erroneous. Such views cannot be accepted; they are erroneous both methodologically and factually.

What would be the correct methodological approach to the problem of the gene from the standpoint of experimental genetics? The gene is an objective reality, a material unit in the living cell. Our task therefore is to estimate correctly the part played by the gene in the life of the cell and to give a prop-

[1] W. Johannsen, *Elemente der exakten Erblichkeitslehre*, Jena 1913, S. 483.

er materialistic explanation for the data that our science has accumulated. We cannot reject the sound and beneficial basis of genetics and disregard the facts obtained by science because this or that scientist has made reactionary statements.

Can we regard the acceptance of the material basis of heredity, i.e., the existence of the genes, as an idealistic conception? The assertion that genes exist should not be misinterpreted in the sense that the material units in the chromosomes, i.e., the genes, are regarded as matter from which the individual characters are built. The gene is not the germ of the character, and not the only responsible material particle of the cell that determines the formation of a character, or that develops into a character. A character is the result of the activity of the cell, of the interaction of the cells and the determining influence of the environment. The gene merely determines the direction in which a character must develop under specific environmental conditions.

Therefore, in determining the course of development and the specific nature of a character the gene is not isolated in its action, on the contrary, it interacts with its environment. To be more precise: the gene plays the decisive role in the hereditary transmission of a character, whilst in the process of development of a character the environment is the decisive factor. Therefore, in this complex system it is difficult to say what is really determinative. When man exerts his control over the development of organisms the environment becomes of primary importance. External conditions help us mould and alter the hereditary basis of an organism.

Thousands upon thousands of experiments have shown that external conditions may influence and change the nature of the gene which in its turn leads to the alteration of a character; the altered character thus obtained becomes hereditary.

It is absolutely untrue that geneticists attribute hereditary transmission of properties solely to the function of the chromosomal complement of a cell and to the minute material units—the genes. Geneticists are charged with asserting that only gene changes may produce altered hereditary properties of the organism. Such a conception has nothing in common with contemporary genetics. It is perfectly true that many geneticists of the early Mendelian period adhered to such vulgar mechanistic

and metaphysical conceptions of the gene, but these were cast aside in the course of the development of genetics as a science. Contemporary genetics has come to directly opposite conclusions which are based on a vast amount of experimental data. I hope I shall not be accused of fawning if I permit myself to cite the American geneticist Muller, who is an undeniably typical geneticist or "formal geneticist" as our opponents are pleased to call it. This is what he wrote in a paper published in 1947:

"... the material is potentially particulate, and each separable part, which determines the duplication of just its own material, may be called a gene.

"Before considering further the properties of individual genes as judged by results of their passage through generations, it should be emphasized that, though particulate in their self-reproduction, their products in the cell interact in the most complicated ways, both with one another and with the products of environmental conditions, in determining the characters of organisms, contrary to what many early Mendelians had assumed."[1]

It therefore follows that not only genes determine characters. Such a view is primitive and incorrect. The definitive character is the result of the development of the cell and of the organism as a whole and is greatly influenced by the environment. In a recent paper (published in the *Doklady of the Academy of Sciences of the U.S.S.R.* LVIII, No. 7, and LX, No. 4) I demonstrated how the penetrance of a hereditary character may be altered under the influence of external factors and how these changes may be controlled. If the genotype is returned to its former conditions the normal penetrance of the character follows.

Geneticists are accused of denying the inheritance of acquired characters; this should be understood as an absolutely just reaction to the primitive experiments of Agnes Bluhm, Brown-Séquard, Krzyzaniecki and others, and not in the sense that the chromosome theory of heredity altogether denies the possibility of change due to the influence of external factors. We are working on this problem and are making efforts to discover

1 H. J. Muller, "The Gene," *Proceedings of the Royal Society*, Series B. Biological Sciences, 1947, p. 5.

the mechanism of hereditary variability. Let us consider the work of Comrade Rapoport (who is an extremely nervous man and does not know how to behave himself in a scientific discussion); he has been doing some very interesting work on the induction of hereditary changes through chemical agents. Rapoport has found such chemical agents that produce mutations in almost 100% of the cases. If I had the time I would refer to the numerous experiments that prove the existence of the gene and that demonstrate its mutability.

The attacks on the view that genes exist reminds me of the earlier denials of the existence of the atom. Although the atom itself has never been seen, no one doubts its existence today. So it is with the chromosome. There were scientists who vigorously denied the reality of chromosomes. We claim that all plants and animals have specific chromosome complements which vary in number from two or three chromosome pairs to several hundred.

In his speech Academician Mitin has demolished chromosomes and genes with such a ferocity, as though there were nothing more dangerous than a chromosome. But even Academician Lysenko, who is most violently opposed to this theory, wrote the following lines in 1947:

"It is true that chromosomes exist. In the gametes their number is half that of what it is ordinarily. If the gametes bear chromosomal aberrations of any kind they give rise to altered organisms. It is true that the various visible morphological changes of a given chromosome often, and even always, bring about changes in various characters of an organism. It has been proved that the presence of two X-chromosomes in the fertilized egg of *Drosophila* usually determines the development of a female from that particular egg and not of a male.

"These facts as well as other data are correct. Beyond doubt, changes in the egg or in its chromosomes cause a change in the development either of an organism as a whole, or of its separate parts; but *it is likewise an indisputable fact that altered environmental conditions may change the process of the building up of the body, including the building up of the chromosomes and of germinal cells in general for the future generation.* In the first case the rudiments (egg), that had been altered by environmental conditions, produce altered organisms; in the

second—the organism, modified by external factors, may produce altered gametes."[1]

I wholeheartedly agree with this statement of Trofim Denisovich; it is absolutely correct without any reserve whatsoever. But permit me to ask (without any desire to take the palm) who discovered this fact? Not Weismann, nor Bateson, of course. These postulates were elaborated and proved by geneticists.

Trofim Denisovich (I should like to have your point-blank answer to this question), if, as you most properly put it, changes in the chromosome produce changes in character, why then not investigate into the mechanism and the nature of the changes produced in the chromosome so as to be able to control them? Why should the statement of Trofim Denisovich that "the various morphological changes of a chromosome often, and even always, bring about changes in characters" be considered correct, whilst the conception of the heterogeneity of the chromosome, the qualitative dissimilarity of its various parts, and of the significance of chromosome numbers in the development of characters (polyploidy) be denounced as downright idealism by Academician Mitin? Moreover, numbers of very exact experiments have shown that genes are strictly localized and that by changing a given locus we infallibly change a particular character. Changes in this locus are always connected with changes in some particular character. This is not mere speculation, this is a fact that can be seen under a microscope.

I therefore once again address myself to Trofim Denisovich, what is idealistic about that? If you consider the existence of the chromosomes and their affinity to the characters of the organism to be a fact, then why not study the structure of these chromosomes? A study of the structure of the chromosome (in your Institute of Genetics chromosomes are studied, aren't they?) discloses its heterogeneity and linear dissimilarity. This has been definitely proved by experiment. I myself have confirmed it experimentally and checked my data cytologically.

In the face of these facts I cannot accept Koltsov's theory

[1] Т. Д. Лысенко, *Агробиология*, 1948 г., стр. 427.

of the stability of the gene which Trofim Denisovich so justly criticized.

Is Koltsov right? No, he isn't. Do the geneticists, who discovered the chemical structure of the nuclear material, its chromosomes, share Koltsov's views? No, they do not.

Having this in mind, I simply don't understand why colchicine and other agents that induce polyploidy, i.e., increase the number of chromosomes in a cell, which in its turn influences the numerous characters of an organism, as a result of the alteration of the genome as a whole, cannot be used for the creation of new varieties, and for the benefit of our agriculture?

Or why should we discard hereditary changes produced in organisms by X-rays and chemical agents, although it is well known that these factors produce the very morphological changes which Trofim Denisovich himself acknowledges, and that by such methods the activity of penicillin, for instance, has been raised a thousandfold? We must make use of them! And if these methods must be used, why not investigate further and elaborate them?

I raised only one problem, the problem of the gene.

It would be worth while to cite a number of other cases, but I am limited in time.

One thing is clear. Facts cannot be discarded. The vast amount of experimental data accumulated for the past fifty years cannot be ignored. It is only necessary that these facts be correctly estimated from the point of view of dialectical materialism and on this basis a lucid theory of heredity should be created.

I have already mentioned that it is high time for our geneticists, who adhere to the fundamental principles of the theory of the gene, to appreciate the significance of a clear and correct theoretical and philosophical outlook for the development of contemporary genetics. Soviet geneticists must understand that the metaphysical conception of the stability of gene and genotype, and of the direct relation between gene and character lead to reactionary conclusions in the theory of biology, and to the fallacies of Serebrovsky, Filipchenko, Koltsov and many other geneticists.

It is quite true that there was not enough self-criticism in our midst, that we were reticent about washing our dirty linen

in public. This led to an accumulation of errors and to the cropping up of such reactionary doctrines as Serebrovsky's idea of the genofund and Filipchenko's autogenesis.

A few words about Mendelism. As you all know, Mendel discovered the laws of segregation in the middle of the nineteenth century, and these laws were re-discovered at the beginning of the twentieth. The essence of this discovery was that the hereditary qualities of the parents do not blend after the fusion of the reproductive cells. They continue to segregate in the following generations and proceed to act independently.

The first account of Mendelism in Russia was made by Academician Pashkevich. Although this was a scientist of some repute in his day, nevertheless his summary of Mendelism was so inaccurate that it totally misrepresented the teaching.

Michurin regarded Mendelism at its early stage in Russia with due mistrust. In 1911 Academician Pashkevich wrote that if a frost-hardy variety of apple with small fruit be crossed to a southern one with fruit of medium size, in the first generation a frost-hardy plant with small fruit will be obtained, whilst in the second generation segregation will take place and the desired new variety is thus produced. A combination of the desired qualities of both parents is thus obtained, and the task of the plant breeder is easily fulfilled.

This was, of course, a wild fantasy. And since Michurin at that time was already well acquainted with the nature of fruit trees, he knew how complicated the heredity of such characters is and he was well aware of the intricate processes that a plant undergoes in its individual development. He could not help regarding the vulgarized approach of the early Mendelists in Russia as utterly metaphysical and absolutely unfounded.

The unique path of Michurin as a scientist should be likewise borne in mind. It is well known that for many years Michurin adhered to Grell's theory which later he discarded as fallacious and unscientific. This could not help make him distrust all sorts of "new-fangled" theories. He only accepted that which his own experience confirmed.

That is why I think it is wrong to cast aside all that Michurin wrote about Mendelism and to claim that he altogether disregarded Mendel's laws. I shall not occupy your time by reading excerpts from the works of Michurin where he in-

dicates how Mendel's laws may or may not be applied. They are well known. I merely want to stress the difference between the views of Michurin on Mendelism and the erroneous ones of the early Mendelists.

Michurin stated that the principal shortcoming of Mendelism is its neglect of the part played by the environment in the development of an organism, particularly that of a perennial. He wrote: "It seems to me that all of our Mendelists are disinclined to take into account the powerful influence of these factors [environmental factors—*S. A.*] on the development of the hybrid plant, beginning with the formation of the seed from the cross of two individuals and continuing during the first years of growth of the young seedling right up to the stage of maturity."[1]

By changing the conditions under which the plant developed Michurin obtained remarkable results. He succeeded in making manifest the properties of the parent plants which were of particular interest to him. In this case external factors cannot be separated from the internal ones, and the proper selection of the parent plants guarantees the successful influence of external factors.

There is nothing mysterious about Michurin's conception of the rearing of an organism.

He wrote: "In particular this influence manifests itself in the mother plant at the time when seeds of the future hybrid organism are being formed, and in the resulting hybrid during its earliest stages of development...."

I call your attention to the following phrase "...favouring some and serving as insuperable impediment for the penetrance of other hereditary characters. And almost always the success in crossing plants solely depends upon such influence."[2]

This postulate expresses the essence of Michurin's conception of the part played by external factors in development. The early Mendelists failed to grasp this fact. This was the weak spot of vulgar Mendelism. I have in mind the underrating of the determinative role of external factors in the development of the genotype.

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[1] И. В. Мичурин, *Сочинения*, том I, стр. 240.
[2] *Ibid.*, p. 338.

That is why it is useless to argue about whether Michurin was for or against Mendelism.

I accept the just reproach that we have insufficiently studied Michurin's heritage and that we have paid but little attention to Michurin's methods. We debate the question who is and who is not a Michurinite, and yet up to this day there is not a single monograph on Michurin published. Why was so little attention paid to the heritage of Michurin, why weren't his works more broadly popularized? The Academy of Agriculture has failed to do this, so have other organizations.

I confess that I am just as much to blame as any of my opponents. Lysenko must be given his due for calling the attention of geneticists to the works of Michurin. I shall not expound upon Michurin's place in biology, I have done that in one of my papers. Here I simply want to stress that a clear-cut summary of Michurin's teachings would do much to advance the application of his ideas to our agriculture. I cannot agree with Lysenko who claims that the cardinal point in the works of Michurin is his teaching about vegetative hybrids. . . .

T. D. Lysenko. Who ever said that? Where did I ever write that?

A voice. Yesterday just the opposite was said.

S. I. Alikhanian. You are always claiming that the teaching about vegetative hybrids is the main point in the works of Michurin.

T. D. Lysenko. You are either intentionally or unintentionally asserting an untruth!

S. I. Alikhanian. I have never done wrong intentionally. I am sincere in everything I do.

T. D. Lysenko. Intentionally or unintentionally you are always putting it so that Lysenko took from Michurin only his teaching about vegetative hybridization. You can find the point anywhere that the cardinal feature of Michurin's teaching (and every Mendelist should get that straight at last) is the role of the environment. That's the point.

A voice. Do you understand now? (*Stir in the audience.*)

S. I. Alikhanian. You interrupted me just when I was about to touch upon the subject. I shall make an effort to answer this point as best as I can.

Being a university teacher I am used to giving marks for the answers I get. I am convinced that Comrade Lysenko will give me a bad one for my answer.

I have never ignored Michurin's teaching neither about vegetative approximation nor about the role of the environment in producing new varieties of plants. This is an extremely important part of Michurin's doctrine, and one must be blind, indeed, not to see that. (*Noise.*)

Unfortunately, I haven't at hand an appropriate citation. (*Stir in the audience.*)

Comrades, in half an hour after I finish my speech I shall be able to present the necessary excerpts to the Presidium. They will clearly show that, according to Lysenko, Michurin placed a sign of equality between sexual and vegetative hybridization. For example, here is an article of Popovsky published in the journal *Novy Mir* in which the author in his account of Michurin's work constantly uses the word "coalescence" instead of "crossing," as though to stress the fact that Michurin dealt only with vegetative hybridization.

A voice. Then tell it to Popovsky.

S. I. Alikhanian. I want to say a few words about Michurin's sharp criticism of Grell's theory as well as about some of Michurin's predecessors and contemporaries.

Grell's principal postulate was that the acclimatization of a plant solely depended upon the wilding.

Elaborating Grell's theory, a certain Guryev wrote the following: "The surest means of acclimatization is to give a southern fruit tree a good hardy wilding."[1]

So as to leave no doubt that Grell had vegetative hybridization in mind as a means of creating new varieties, although the method was called in those days "the scion-stock" theory, I must cite the following passage: "Finally, the typical influence of the wilding on the scion becomes clearly manifest in the offspring, i.e., when a new variety is obtained from the seeds of the fruit from a grafted tree of this particular variety."[2]

This is what Cherabayev wrote on the subject:

"The influence of the sap of the wilding on the embryo in the seed is fully confirmed by the fact that the taste of the

[1] Русское садоводство, № 24, 1900 г.
[2] Русское садоводство, № 209, 1907 г.

fruit of a tree grown from seeds obtained from a grafted plant in the majority of cases is similar to or differs slightly from that of the wilding."

The main principles of this theory, laid forth in the papers of Grell and his followers, were printed in all the Russian journals dealing with fruit and flower gardening from 1860 to 1914.

In order to change the hereditary properties of a fruit tree, to remodel it, that is, to acclimatize the southern varieties in the North, it was advised to graft the southern variety onto a northern one. Under the influence of the frost-hardy host the grafted variety acquires this new character, yet at the same time preserves its flavour. Moreover, these qualities are passed on to the seeds. Such was Grell's theory of acclimatization.

It is well known what Michurin wrote about these fabrications of Grell and of his own early enthusiasm about Grell's works. Later he waged a bitter struggle against Grell and his school and showed it to be ineffectual and aimless. This is what he wrote about his controversy with Grell:

"Unfortunately, I at first was enthusiastic about Grell's ideas of those days concerning the acclimatization of the best foreign varieties by grafting them on to hardy wildings. Much labour and time were lost on these erroneous experiments before I finally became convinced that the method was useless."[1]

So that there may be no doubt about Michurin's disapproval of the "scion-stock" method, I shall cite what Michurin wrote about one of Grell's disciples, Cherabayev:

"Just examine it, please, [Michurin is referring to Cherabayev's paper] there is certainly something very incongruous about it. In his opinion, the stock has an influence on positively every part of the variety grafted on to it: on growth, on fruit bearing, on the shoots, on hardiness and, lastly, on the formation of the seed, yet, suddenly an unexpected exception: when it comes to the quality of the fruit, he does not recognize this influence as operative. Say what you like, but it is hard to agree with this view, all the more that this is not the way it works out in reality."[2]

After all that has been said, I should like to know, Trofim Denisovich, how does this conform with your statement that

[1] И. В. Мичурин, *Сочинения*, том I, стр. 90.
[2] *Ibid.*, p. 143.

"by planting the seeds from the scion or the stock it is possible to obtain offspring, individual representatives of which will possess the characteristics not only of the strain from which the seed has been taken, but also of the other with which it has been united by grafting."[1]

I simply want to be clear on this point, and I should like to hear your explanation. It seems to me that there is some contradiction between your views and those of Michurin on this matter.

A voice. You ought to get a bad mark here.

S. I. Alikhanian. Well, I wouldn't give you a better one for your bad behaviour. (*Laughter.*)

What does Michurin say?

". . . By means of grafting I attempted to bring the South to the North, believing that the southern varieties grafted on to our frost-hardy wildings would adapt themselves to our climate with greater rapidity, and that their seeds would produce seedlings from which, after being exposed to the influence of various factors, new improved varieties might be selected. But alas, here, too, I met with failure; all of my seedlings were killed by the frost during the first winter."[2]

That is why I am of the opinion that it is quite right to speak of the influence of the host on the graft such as Michurin showed it to be in his mentor method and his experiments on vegetative approximation. That is why I think it would be wrong to consider Michurin's methods a mere repetition of Grell's ideas. The passage from the works of Michurin which I cited above clearly shows that this is not so.

As a matter of fact the majority of Michurin's varieties were obtained by means of sexual hybridization and selection, and only in some of the cases was the mentor method used to make manifest properties, which Michurin combined in the hybrid by means of sexual hybridization.

In conclusion I should like to say the following. It is wrong to disregard all the facts accumulated by our science; neither the advance of science, nor our practical work can profit by it. I refer to our agriculture and to the field of medicine.

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[1] Т. Д. Лысенко, *Агробиология*, стр. 432.
[2] И. В. Мичурин, *Сочинения*, том I, стр. 429.

V. V. Sakharov, who had worked for many years on the problem of the gene is now successfully working on the production of a new variety of tetraploid buckwheat. M. S. Navashin has already produced a tetraploid kok-saghyz which has been sown on a large scale. The well-known cytologist Khajinov is successfully tackling the problem of utilizing inzucht-hybrid seeds of Indian corn. Astaurov's brilliant work with the silkworm is universally known, so are the remote wheat hybrids produced by Tsitsin, Pisarev, Khizhnyak as well as the work of many other Soviet geneticists in the various practical fields of our national economy. I can name many of our greatest plant breeders who have produced varieties that are now sown on enormous areas amounting to millions of hectares of our socialist farms. There is Shekhurdin, for example, whose spring wheats are grown on an area of 10,000,000 hectares, Academician Konstantinov whose 15 varieties of wheat, barley, maize and millet occupy an area of over 5,000,000 hectares, the late Academician Lisitsyn whose varieties are grown over an area of 5,000,000 hectares of collective-farm land, Academician Yuryev, Professor Uspensky and many others whose varieties are planted on millions of hectares. These scientists did not work on theoretical problems as Sakharov, Navashin, Glembotsky and Astaurov had done, but they successfully applied in their work the data which our science has obtained. I deeply regret that a great many geneticists who could tell of this successful work in the altering of the nature of plants and animals are not present. I can name a number of scientists whose presence at this session would have been very helpful in giving an idea of the work that is being done.

In conclusion permit me to say a few words in answer to Academician Belenky. You stated that Alikhanian promised in 1939 to produce a new breed of chickens, and that he failed to keep his promise because he had based his work on false premises. I must say, Comrade Belenky, that before making such broad statements one should know how things really stand. After three years of work on the production of a new breed, in 1941, gun in hand, I went to defend my country. I returned five years later, having lost a leg in the war. Due to ill health I could no longer work with chickens. And even if my health had permitted, I could not continue the work be-

cause my entire stock (100 birds) was gone. I am sure Comrade Belenky will agree with me that I was not wrong in giving up the work with chickens and going to fight for my country. Nevertheless, if I were given the chance in 5-7 years I could repeat the work I had planned with the Leghorns.

Zinovi Yakovlevich Beletsky! Your speech surprised me, indeed. You have certainly misrepresented everything. I should have never believed it possible for you to misinform the audience so. You were treated decently at the Moscow University. Not having a degree you pleaded for time to work on your dissertation, and both the administration and the Party organization granted your wish and gave you a two months' leave. Zinovi Yakovlevich, it is wrong to slander our University so.

Having rid our science of all idealistic rubbish and cleared it of the false idealistic conceptions of bourgeois geneticists as well as of those to which some of our own scientists had adhered, we, Soviet scientists, in an atmosphere of friendly and constructive discussions, must resolutely sponsor the advance of Soviet science, and make use of all that is creative in our science for the good of our country.

We must further most indignantly declare that the attacks of English Darlingtons and American Saxes on our Soviet science have nothing in common with a scientific approach. Our wonderful mother country is a mighty stronghold of peace, the pride and hope of all of progressive mankind. We Soviet scientists are a part of the Soviet people. There may be controversies in our midst, because constructive discussions sponsor the advance of science. These debates must be based on a moral and political unity, must be conducted on the foundation of the one and only progressive doctrine—the teaching of Lenin-Stalin. This is what unites us, is very dear to us and serves as a guarantee that our Soviet science will score new victories for the good of our country. (*Applause.*)

P. P. Lobanov. A 15-minute intermission is announced.

Voice from the audience. There are some written questions to be answered.

S. I. Alikhanian. There are so many that I must have time to read them.

Academician P. P. Lobanov. The proceedings of the session are resumed. I call upon Comrade Alikhanian to answer the written questions. Is Comrade Alikhanian here? No. In that case, I call upon Professor Polyakov.

Professor I. M. Polyakov (Corresponding Member of the Academy of Sciences of the Ukrainian S.S.R.). An intense struggle is raging in the field of biology today between the progressive and the reactionary, between the old and the new, between materialism and idealism. In this struggle, the progressive, scientific ideology of the Soviet Union stands opposed to the putrid ideology of the capitalist world. The struggle that is going on in the field of biology is a reflection of the acute, intense class struggle.

How is this struggle concretely reflected in the theory of evolution and genetics?

If we are to speak of the external aspect of the matter, we see that in Western Europe and in America, anti-Darwinist theories and propositions of different kinds, all sorts of aristogenesis, protero-genesis, and so forth, are cropping up and multiplying in great number. The same may be said about these "geneses" and "isms" as what Lenin said about the *Gelehrter* juggling with new catchwords, ingenious terms and subtle isms. The object of all this pseudo science is to screen the philosophical line that is hostile to us, the line of idealism and metaphysics.

In the theory of evolution and genetics, the attack on Darwinism is at the present time being waged mainly by Neo-Darwinism, by Weismannism.

Neo-Darwinism has proved incapable of solving fundamental problems of the theory of evolution like the correlation between external and internal factors in variation and evolution, the correlation between the part and the whole in the evolution of organisms, and so forth. Weismannism-Neo-Darwinism has built up a metaphysical theory of evolution which denies that natural selection plays a creative role, and incorrectly treats of the role of hereditary variations, the significance of mutations in evolution, and a number of other problems.

Morgan, Simpson, Griggs, Shull, Mayer and others, all, or many of them, regard themselves as Darwinists, but in essence,

they are Weismannists, Neo-Darwinist reactionaries. And it has been quite rightly and opportunely stated that many present-day authors who call themselves Darwinists are, in essence, metaphysicians, Neo-Darwinists, they are—to take the old term Mendelian that Timiryazev used—Neo-Mendelians.

In their treatment of the processes of evolution, the Neo-Darwinists assert that mutations play the dominant role; they deny that external environment influences the process of evolution. Weismann, Morgan, Shull and the others, all regard the organism separately from environment, substitute segregation for the creative influence of selection (Mayer and others are examples of this), assume the possibility that under conditions of isolation, by means of segregation, mutations may occur not only of lower systematic categories, but also of higher categories. The result of this idealistic, metaphysical conception—which follows not from Darwinism, but from present-day Weismannism, from a wrong appraisal of the role of mutations in evolution, from a wrong conception of the interrelationship of the organism and environment in evolution—is that all these authors join up with the autogeneticists. On the other hand, the anti-Darwinists are joined by the Lamarckists, who take a vulgar view of the interrelationship of the organism and environment, stand by the vulgar theory of equilibrium, and are unable to give a scientific, materialistic explanation of the problem of organic fitness.

It is no accident that many bourgeois geneticists have gone over to Weismannism and Neo-Darwinism in the matter of the theory of evolution, that they are now advocating the theory of preadaptation, according to which the organism acquires adaptational features long before it comes under the given environmental conditions, and that these features are revealed only under suitable conditions of isolation.

It is no accident that Morgan, Shull and others, take their stand on the theory of preadaptation. It is no accident that Punnett, only very recently, tried to revive Batesonism, and that Griggs, Nabour and others, are reviving Lotsyism, which may seem monstrous in our times.

This is due to a number of grave theoretical errors in genetics itself. Wherein lie these errors? In my opinion, they lie primarily in the theory of autogenesis, in the failure to

understand the physiological nature of the process of hereditary variation. These errors are due to the metaphysical conception of the gene, to failure to conceive of the organism as an entity in individual and historical development. This inevitably led to an abstract and profoundly abiological approach to the problem of speciation so characteristic of the scientists mentioned. Hence their incorrect treatment of mutations, their preadaptationism, and all the other concepts of anti-Darwinism.

Hence, it follows, that many present-day geneticists abroad who call themselves Darwinists are really not Darwinists but Neo-Darwinists, Weismannists, i.e., metaphysicians, anti-Darwinists.

Among us these problems are very acute, for they are not abstract theoretical problems concerning which a lot of nice, scientific words can be uttered; they are all problems that are closely connected with life, with the practical work of producing new varieties, in which our country is exceptionally interested. Our country is confidently marching along the road towards Communist society and, naturally, it is the duty of our scientists to utilize everything in science that will facilitate the march to this glorious goal and ruthlessly cast aside everything which to any degree hinders, which does not ensure that the march proceeds along the most fruitful, the most productive road. Hence the necessity of fighting for clear and distinct positions in science, positions that are in harmony with facts, and hence, in harmony with the dialectical materialist conception of living nature.

I want briefly to express my point of view on some of the fundamental problems that have been touched upon in the course of this discussion.

Our position in respect to the theory of evolution must be the position of Darwinism. I do not employ the term "orthodox Darwinism" that is ascribed to me. I have always written that Darwinism, as formulated by Darwin, contains a number of postulates that stand in need of revision; Darwin's theory contains a number of fallacious propositions, and so there is no purpose in calling ourselves orthodox Darwinists. But we are Darwinists, because we agree with the materialistic kernel of Darwin's theory, with its profoundly creative, profoundly correct theory of natural selection, without which it is impos-

sible to explain the main features of the organic world and the adaptability of organisms to environment, what is called organic fitness. If we categorically reject Lamarckism, it is not, of course, because (this must be stated clearly) Lamarckism raises the problem of the role environmental factors play in variation, but primarily because the Lamarckists attempt to solve the fundamental problem of the theory of evolution from an unscientific standpoint. The Lamarckists' answer to the problem of the correlation between the organism and environment is wrong. In essence, Lamarckism is the vulgar theory of equilibrium, and this inability to give a materialist scientific explanation of the phenomenon of fitness is highly characteristic of Lamarck himself, and of the Lamarckists who followed him.

It is no accident, therefore, that the theory of initial purposiveness is the characteristic feature of Lamarckism. Characteristic of Lamarckism also is the theory of the inherent striving of organisms for progress, the principle of gradation. All these idealistic concepts spring from the extreme vulgarization, from the incorrect general conception of Lamarckism, from the conception of present-day Lamarckists who deny the creative role of natural selection, without which evolution cannot be explained.

In our discussions the controversy centres mainly around the question of the role of environment in evolution. It is, indeed, the fundamental, or one of the most important of the fundamental problems.

Environment is of extremely great significance in the evolution of organisms. This follows from the very nature of the organism as such. The most characteristic property of an organism is metabolism—its exchange of substances with its environment. What does that mean? It means that the organism cannot exist unless it absorbs, unless it assimilates the environmental factors. That is why environment plays a vital role in the evolution of organisms. To regard the evolution of the organism separately from environment means slipping inevitably to the position of metaphysics and idealism.

But I think it important to emphasize that the influence of environment, the nature of environment, can be interpreted in different ways when we speak of evolution, of the individual development of the organisms, of variation, etc. Let us take

hereditary variation. In its nature, hereditary variation of the organism is physiological. This statement by Timiryazev and Michurin, the statement upon which T. D. Lysenko insists so strongly at the present time, is absolutely correct. It means that variations in an organism can take place only as a result of an alteration in the material structure of the plasm, in the cytoplasm, of the nuclear plasm, of a change in the metabolism that is specific to a given organism, the metabolism that is characteristic of each species. This is the actual way in which the external factors become incorporated in the organism.

This is not the first time that I am formulating this point of view. I have repeatedly written and spoken about it since 1934, in particular, during the discussion organized by the editors of *Pod Znamenem Marxisma* in 1939. If I had time I could cite a considerable amount of material to confirm this.

At the present time the adherents of often different trends in genetics are approaching the point of revealing the physiological nature of hereditary variation. At one time, numerous experiments were popular in the field of genetics by which geneticists hoped to prove that changes in the organism are dependent upon environmental conditions. Among these may be mentioned the numerous experiments with X-rays, etc. These experiments have given way to others by which attempts are being made with the aid of specifically characteristic action to delve deeply into the structure of the organism, into its biochemistry, into its physiology, and to obtain variations in this way.

In this respect, the work of deciphering the hereditary basis of the organism that is being conducted by the Michurin trend is worthy of great attention. To approach this work dogmatically, as some geneticists do, to shut one's eyes and say it cannot be, means turning away from the path of progress in this branch of science.

I, however, believe, that the object of the numerous experiments in which the researchers are trying by means of chemical action—with aminoacids and a number of other substances—to reach a solution of this problem, the work of a number of authors who are trying by means of immunological action, sometimes even responsive action, to change the structure of the heredity of the organism, the object of all these experiments

is (some directly, some indirectly) to delve deep down into the physiology of the organism and to change its hereditary basis.

In this connection it is necessary to touch upon the problem that has been raised here, namely, the inheritance of acquired characters. Very often, many authors link this concept of the inheritance of acquired characters with Lamarckism.

This is correct in the sense that the recognition of inheritance of acquired characters is an essential feature of Lamarckism. But it would be utterly absurd and wrong to say that every investigator who asserts that acquired characters can be inherited is for that reason a Lamarckian, for Lamarckism is an entire conception of evolution that is linked with the idealistic conception of initial purposiveness that leads to autogenesis, but the problem that we are discussing now is a concrete one.

How should this problem be solved? What do I think is the solution of this problem?

A very broad conception of the inheritance of acquired characters is now becoming widespread.

The argument is that variations called forth under definite conditions of existence, under definite environmental conditions, variations caused in the organism (in a number of cases this problem is quite concrete), depending upon the degree to which they can affect, influence, etc., the sexual elements, such variations may be found in the next generation, i.e., may be inherited. In this very broad conception we admit that external factors enter into the process of hereditary variation, and it is on this broad plane that we must speak of the inheritance of acquired characters.

But the main issue in this controversy is not this, but whether characters acquired by certain organs of the body correspondingly change the elements by means of which the organism propagates its species (the sex cells, say); can these responsive variations be directed, be transmitted from generation to generation?

I think, and I have been urging this for many years, that it follows from the very essence of the conception of the evolution of heredity that in the overwhelming majority of cases they cannot. Why? Because the conditions for the formation of sex cells, the conditions for the formation of the elements by means of which the given organism propagates its species,

and the conditions which give rise to variations, for example, in the skin of a salamander in Kammerer's experiment, or in traumatic injury experiments, etc., are totally different things.

That being the case, we cannot conceive of any such corresponding transmission of an alteration in an organism.

Does this mean that such correspondence is impossible in general? No, it does not, because we must very concretely put the question: which organism, which characters, how are these characters connected with the conditions of development of the sex cells, what is the biochemical nature, the biochemical basis of these cells?

Here is a curious fact. A book came into my hands written by Sturtevant, one of the pillars of present-day classical genetics, one of Morgan's assistants. I was very much surprised to find that even this author admits the possibility of the corresponding transmission of changes arising in a definite part of the body (in this particular case, the transmission by heredity of defects in the crystalline lens).

T. D. Lysenko. This was mentioned here.

I. M. Polyakov. I did not know this. This author believes that such immunological actions can give rise to such a variation.

I mention this fact in order to show that this problem must be approached with an unprejudiced mind. At definite stages of evolution, particularly among the lower organisms, among plants, in respect to a number of characters it is quite conceivable, bearing in mind the concrete paths of development of characters in the organism, that these characters should be inherited in this way. But in my opinion, this cannot be elevated to a general rule.

This is not the main issue, however.

In its general form the problem presents itself in this way: environment, the external conditions under which the development of the organism proceeds, enters into the process of variation; through metabolism, the process of variation turns out to be physiological in nature.

It is in this sense that I understand this theory. In this field a vast amount of experimental work has still to be done that will explain much to us and provide us with perfect methods of altering animal and plant forms.

To this day most animal and plant forms are produced by

selection, as in the time of Darwin who resorted to the method of natural hybridization. The problem that was set, and which Michurin solved, was that of directed transformation, of deliberately changing the nature of the organism. But one other fundamental problem must be put. When man directs variation, he obtains what he needs, what is adapted to definite natural conditions, or meets his economic requirements.

If, in nature, this process is spontaneous, then, from the point of view of the general adaptability of the organism as a whole, the variations that take place may produce different effects.

In this case, therefore, the creative role of natural selection comes into play, but this is inconceivable if it is divorced from variation and heredity; it cannot be depicted like a sieve which sifts the mutations that arise. Whether the given variations are fit, adapted to life, or whether they will be discarded in the process of evolution is decided by natural selection. This is the fundamental issue between Lamarckists and Darwinists.

If we take it that environment by itself causes adaptive changes in the organism, it will inevitably lead us to theology.

When we say environment we commit a common error; it suggests temperature, the mineral composition of the soil, water, etc. But, as Darwin himself emphasized, the organism's chief environment consists of other organisms, the biological environment. And so, if the external factors, which enter into the process of variation and are an essential condition for the rise of this or that variation, call forth the appearance of diverse characters, the evolutionary value of this diversity may vary considerably in relation to the biological environment.

For example, if certain butterflies acquire a protective colouring to protect them from foes like lizards and birds, the variation in each butterfly is, of course, due to the influence of environmental factors, it is physiological in nature. There can be no doubt about that. Here it is the influence of the external factors. But all this is indefinite. All these variations will be of the indefinite variation type.

T. D. Lysenko. Can it be predicted or not?

I. M. Polyakov. Michurin only raised the question of variation. My idea is perfectly clear. I want to say that, entering

the process of hereditary variation, environment causes diversity in the direction of variations.

T. D. Lysenko. Can it be predicted or not?

I. M. Polyakov. It is hard for me when I am interrupted.

T. D. Lysenko. It is hard for me when I have to listen to wrong statements. Can it be predicted or not? If, for example, we place a cow under the proper conditions, will her udder grow or not? Can that be predicted or not?

I. M. Polyakov. I say that variations that arise under the elemental conditions of nature proceed in different directions because of the diversity of the organisms and of the conditions surrounding them, and this gives rise to diversity in the direction of the variations. Natural selection decides what is fit and what is unfit.

I am speaking of indefinite variation....

A voice. You criticized....

I. M. Polyakov. I understand the problem somewhat differently from the way you do. Do the causes change or not? Can we control this or not?

This is clear to everybody.

It is in this sense that I interpret Darwin's and Timiryazev's statement about the "indefiniteness" of natural variation—precisely from the standpoint of the adaptive effect, from the standpoint of the general fitness of the organism as a whole. That is how I understand the essence of this problem. I do not say that since there is variation, it cannot be directed.

T. D. Lysenko. All that you have said is somewhat hypothetical. You do not recognize mutations. But only recently you subscribed entirely to Schmalhausen's conceptions. You wrote a most laudatory review of his book. But in it he writes exactly what you expounded here, quoting foreign names and calling it classical genetics (only you did not say of which class). Literally, only the other day you subscribed to the position taken by Schmalhausen. Why are you silent about that?

I. M. Polyakov. Why are you in such a hurry, Trofim Denisovich? I have here before me the synopsis of my speech, and I can show you that I intend to speak about Schmalhausen, so there is no need whatever to interrupt me, and no need to be in a hurry. (*Laughter, commotion.*)

Well, I want to say the following. When we say indefinite

variation, we have in mind, primarily, the question of fitness and of the adaptive effect. From this it follows—and it is important to emphasize this—that since variations in organisms proceed in different directions (I am speaking of natural evolution), there must be biological diversity among the individuals within a species, and this is where I disagree with you, Trofim Denisovich.

Intraspecific contradictions arise from this biological diversity among the individuals that constitute the species and not from Malthusian overpopulation. If I support the theory of the intraspecific struggle for existence and see no reason why the problem should be presented as Trofim Denisovich presents it, it is not because I think it is a Malthusian theory. I have been opposing Malthusianism in all its forms, I have been opposing eugenics, racism and social-Darwinism for twenty years. I have also spoken about the errors in Darwin's conception, in his theory of the struggle for existence. But in my opinion, the struggle for existence in all its diverse forms is due to the biological diversity among the individuals that constitute a species. Intraspecific contradictions which are sometimes not of an antagonistic character, are inevitably, in my opinion (even though under temporary and relative conditions) transformed into antagonistic changes which assume different forms of the struggle for existence, the premise for natural selection. This was a very acute issue during the discussion; the theory of intraspecific contradictions need not necessarily be linked with Malthusianism. We must make a complete study of the intraspecific contradictions that find expression in the struggle for existence, and it is not a matter of Malthusianism. I think complete clarity must be introduced in the problem.

T. D. Lysenko. With reference to clarity. Who fails to see that the question of intraspecific struggle and competition is not only a second rate but even a third rate question in our controversy? But you anti-Michurinists are always bringing it up. Tell me, when did Lysenko ever raise a debate about intraspecific competition? Consequently, the issue is not a third rate question, but, Comrade Polyakov, the significance of environment for the organism, the evolution of variability. That is the issue. Schmalhausen denies all this. You unambiguously

associated yourself with him in the press, but here you talk about other things.

I. M. Polyakov. Permit me to say that of the thirty-five minutes I have been speaking I devoted four minutes to this question, so it cannot be said that I am inflating this question now. I will deal with one other problem and with that conclude.

I think that we must conduct our offensive against nature on a very wide front and take advantage of all opportunities, utilize all the means at our disposal. The leading, chief and extremely fruitful ideas in the science of genetics are those that were clearly formulated by Timiryazev and Michurin, and which foreign reactionaries are doing their utmost to decry. The theory of the selection of pairs in crossing that was elaborated by Michurin, his splendid methods of distant hybridization, his theory of the mentor, training and selection, etc., all constitute an immense program of extremely fascinating and fruitful work. To me they are not merely theories and abstract convictions. I am not making obeisances here in this rostrum; for the past few years I and my close collaborators have not been working on abstract problems of formal genetics, but have been studying the important problem that Michurin raised in all its scope, namely, the problem of selective fertilization; we have been working experimentally. There is no time to speak about this in detail, I will say only a few words about it. We have been able to demonstrate on a number of objects the extremely wide significance of selective fertilization, to establish the connection between this phenomenon with the stages of evolutionary intraspecific divergence; we have succeeded, I think, in analyzing the physiological mechanism of the phenomena, of the processes that take place in the pollen mixture on definite maternal plants, and in showing how small quantities of the necessary pollen that get into the pollen mixture often acquire great significance in the process of fertilization, and we have cleared up a number of other problems.

I am saying all this in order to emphasize that to me, the problems of Michurin genetics, the Michurin theory, are not abstract problems remote from me, and to which I want to make diplomatic bowings and scrapings. It is the field in which I am working and which interests me most. It is a fascinating

field and makes me ponder very deeply over a number of important problems of evolution and genetics.

I want to say that I have not been and am not now engaged in any other branch of genetics, but I think that a number of important trends in the experimental study of problems of genetics ought to be developed. I regard as interesting everything that is connected with cell physics and chemistry, with cytochemistry, in connection with a number of problems of genetics. I regard as important everything that is connected with the problems of the inheritance of sex, particularly among lower organisms. I regard as important and interesting experiments that show through which physiological processes the hereditary potencies of an organism are realized. I regard as very important, both from the theoretical and practical point of view, researches connected with polyploids, and a number of other researches.

In my opinion, genetics and Darwinism should develop on a wide front in our country, and we need a number of trends that would discover interesting and important facts. These should not be brushed aside. These trends should not be treated so lightly.

Lastly, I want, and feel obliged, to speak about Schmalhausen. The accusations that have been made against me arise from the fact that I have favourably reviewed Schmalhausen's works. I deem it my duty honestly and frankly to say what I think about this now.

During the past few years, not counting his experimental works, Schmalhausen has published four books: *Factors of Evolution, The Organism as a Whole, The Ways and Laws of the Process of Evolution,* and *Problems of Darwinism.*

Are there flaws and errors in these works? Yes. I have pointed to a number of them, other comrades have pointed to others, and some of the problems he raises are worth a special serious discussion. But I want to say that Schmalhausen's works must be discussed calmly. They are not a short newspaper article of no value that can be thrown out of the window. All that is good in those works should be utilized.

What is good in those books? The study of cardinal problems like "the organism as a whole in evolution," the ways and trends of adaptive evolution, and a number of others, in

which Schmalhausen is continuing the sound Severtsov trend. Nobody will convince me that this is not so.

In this book we find a sharp and profound criticism of various anti-Darwinist conceptions—Neo-Darwinism-Weismannism, holism of autogeneticists, and others. Why should we shut our eyes to that?

There are controversial matters here, and we ought to discuss them. Quotations were read here from his major work, which happens to be the least successful of his books, namely, *Factors of Evolution.*

T. D. Lysenko. You wrote a laudatory review of this, in your opinion, least successful of his works, and recommended it for a Stalin Prize!

I. M. Polyakov. Yes, I think that nearly all of Schmalhausen's works are....

T. D. Lysenko. Did you recommend it for a prize?

I. M. Polyakov. It was recommended. It was recommended because there is much that is good also in this book.

A voice. You expressed an official opinion!

I. M. Polyakov. Not only an official opinion....

A voice. Not on all his works, but on *Factors of Evolution.*

I. M. Polyakov. In *Sovietskaya Kniga* I expressed an opinion on all three books of Schmalhausen's including....

A voice. Did you write officially?

I. I. Prezent. Speak more straightforwardly, more precisely.

I. M. Polyakov. What about?

I. I. Prezent. Speak so that everybody may understand what your point of view, your position is.

I. M. Polyakov. My position is this: Schmalhausen's works contain debatable passages, erroneous....

A voice. Which?

I. M. Polyakov. ... but in Schmalhausen's works, on some points, there are a number of ideas important for the theory of evolution, the study of which should not be brushed aside; they are based on an immense amount of biological, embryological and ecological material, and are very important and interesting. Why should we ignore it?

I have heard here that Schmalhausen is being accused of supporting the limits idea, the idea that there is a limit to variation, etc., and a passage was quoted from his work. I have read

this passage and I must say that it gives no grounds for such an accusation.

T. D. Lysenko. Do you really agree that the udder of a cow which now gives 60 litres of milk per day, was predetermined while still in the wild state, as it appears to be according to Schmalhausen? Do you share that point of view?

I. M. Polyakov. That's nonsense.

T. D. Lysenko. Why didn't you say that in your review?

I. M. Polyakov. I don't interpret it that way....

T. D. Lysenko. That's what it says: previously accumulated, before domestication....

I. M. Polyakov. I interpret it differently. Schmalhausen writes that, in essence, the possibility of variations is unlimited.

I want to say that there are undoubtedly a number of debatable points in our science and we ought to clear them up by joint, comradely, constructive criticism. In this respect, we must, irrespective of persons, deal sternly with mistakes, with deviations from the main line, and this constructive discussion will help us to formulate the correct point of view.

I think it is wrong, and of no benefit to our work, to stick labels on people in lieu of criticism, as is often done. A man may have been fighting Weismannism for fifteen years, but if he happens to disagree with Academician T. D. Lysenko on the question of the struggle for existence, he is labelled a Weismannist. I am not an advocate of "vegetarian relationships" and I am not afraid of sharp talk and criticism, but I think that the task of our science is to unite all that is sound in science around the main, leading trend, and I think that calm, comradely criticism is much more likely to accomplish this than bullying and sticking labels.

I am convinced that as a result of this discussion our Soviet science, all our scientists, will carefully ponder over all that is taking place here, that they will fight for the further progress of our science, for its advancement, for bold criticism, for self-criticism. There can be no doubt that our science, united around the progressive ideas which in this field are associated with the name of T. D. Lysenko, will achieve even greater success than it has up till now.

T. D. Lysenko. You speak about the Michurin trend. But

you wrote nothing about it in your review. When speaking about Schmalhausen's *Factors of Evolution* you did not say whether you think it right that Schmalhausen did not say a word about Michurin and Timiryazev, either in the text of his book or in his catalogue of literature. Do you think it is right? A second question. You call for criticism and self-criticism to prevent stagnation, and so forth. That is all very well. Do you know a scientist who only recently said and wrote: I have wavered a long time, not knowing which side to take. Formerly I was closer to the Michurinists, later I firmly decided to support the Schmalhausen side—but now you are with the Michurinists? (*Applause.*)

I. I. Prezent. I have the following question: Ilya Mikhailovich pleads here for discussion without bullying criticism. In this connection I would like to put the following concrete question. At the conference on Darwinism, and at the preceding conference on genetics at the Moscow University, a resolution was carried in which Academician T. D. Lysenko was called a Lamarckian. Did you vote for that resolution?

I. M. Polyakov. The question I am asked is: have I altered my opinion on problems of the theory of evolution since the conference on Darwinism? At that conference I spoke on two questions: on the intraspecific struggle for existence, and on this question I have not altered my opinions, as I stated here; the second time I spoke on the experimental work I was doing, but this is not under discussion here.

Then there is the question: Do I think that Dubinin's hypothesis on genetico-automatic processes is idealistic? This hypothesis is one of the varieties which I here qualified as mutationism, or Neo-Darwinism, so that I spoke about it. I could not mention all the authors.

As regards the position of Academician T. D. Lysenko and Lamarckism. In 1939, at the conference called by the editors of *Pod Znamenem Marxisma,* I said in this connection that the fact that a man raises the question of the role of the external factor in variation is no reason why he should be qualified as a Lamarckian. In his address at this session, Academician T. D. Lysenko said that the Michurin trend goes neither the way of Neo-Lamarckism nor the way of Neo-Darwinism, and that the Michurin trend must dissociate itself from Neo-La-

marckism and Neo-Darwinism. I can only heartily welcome that statement.

But there must be absolute clarity on this question. I would like to know how organic fitness is interpreted here. What are the place and role of natural selection? These questions are decisive.

I. I. Prezent. You have not answered my question about the resolution.

I. M. Polyakov. What did the resolution say?

I. I. Prezent. Ilya Mikhailovich has suddenly forgotten a most interesting resolution which stated that T. D. Lysenko had slipped into the unscientific, anti-scientific position of Lamarckism. It was stated that the resolution had been "adopted unanimously," and the list of those present contains your name. I would like to know in this connection what your actual position was when the vote was taken.

I. M. Polyakov. I will not conceal the actual position I took. I was of the opinion, and I said so in my speech, that in relation to Lamarckism and Darwinism, T. D. Lysenko, when speaking on the question of the intraspecific struggle for existence and natural selection, tried to combine the two positions. That is what I said at the conference. As for the statement that the Michurin trend goes neither the way of Neo-Darwinism nor the way of Neo-Lamarckism, I can only welcome it.

Academician P. P. Lobanov. I call upon Academician P. M. Zhukovsky.

Academician P. M. Zhukovsky. Our disagreements centre mainly around two questions: firstly, the chromosome theory of heredity, and secondly, the influence of external conditions. Trofim Denisovich Lysenko insists on a direct answer to these questions.

As regards the chromosome theory of heredity. It would be deplorable if the entire group of geneticists that has been labelled Mendelist-Morganists were, from this rostrum, to renounce the chromosome theory of heredity. I do not intend to do that. The cardinal fact for me, a student of the plant world, is the alternations of generations in the plant world, accompanied by changes in the nuclear phases. Beginning with the lower plants

—the green, brown and red algae, through the fungi, the mosses, the ferns, the gymnosperms and the angiosperms—we see a single, evolutionary, rhythmic alternation of generations, accompanied by a change in the nuclear phases.

In plants we see two generations, namely, an asexual and a sexual generation. As a rule, the life cycle of each plant is based on the alternation of these two generations. The asexual generation ends with the production of spores, and in the spore-producing process a reduction division of the nucleus takes place: the diplophase gives way to the haplophase, and the spores are always haploids, i.e., contain half the number of chromosomes. The spore grows into the sex generation, the whole life cycle of which takes place in the haplophase, i.e., all the cells of the given generation contain a half set of chromosomes. In this sexual generation the gametes are formed, which, on fusion, restore the diploid phase in the zygote.

The zygote is the embryo, which develops in the diploid generation (sporophyte).

It is simply impossible to deny this. The alternation of generations is a fact. More, it has been verified by evolution in nearly all the representatives of the plant world, and it has been fixed by evolution.

True, when we take the animal world, we do not see the alternation of diploids and haploids so clearly; but even here, as a rule, the reduction of the chromosome takes place before the sex gametes are formed. Both the male and the female sex gametes have half the number of chromosomes, which, on fusion, form the diploid phase—the zygote, the embryo, which develops in the organism with diploid tissues.

Thus, the single idea of the alternation of the haplophase and the diplophase runs through the animal world too. The basis of it is the single and double sets of chromosomes. First we have half the number of chromosomes, then the double number.

If this were not so, the number of chromosomes would multiply from generation to generation and would reach billions, and such organisms would have been swept from the face of the earth by natural selection long ago.

We have also investigated the cases when there is no alternation of generations in a plant. It is known as apomixis, a

phenomenon in which there is no sexual process, when the embryo develops from other cells of the embryo sac. But in such a case there is no sexual process. It is excluded from the life cycle.

Recently, the Saratov scientist Khokhlov advanced the idea that today the evolution of angiosperms is proceeding in the direction of the liquidation of the sexual process, and he has even proposed that a new group be singled out—an asexual-seed group of angiosperms as having the highest prospects in future evolution.

I do not think that the anti-Mendelists also believe that evolution is proceeding in the direction of the liquidation of the sexual process.

This, primarily, is the significance of the chromosomes in the development of the plant world, and also of the animal world, ensuring the alternation of generations—sexual and asexual—which engenders the alternation of the nuclear phases. The life cycle is based on the reduction division of the nucleus, which is a most important evolutionary phenomenon. Reduction division has been proved by evolution to exist in all representatives of the plant world.

In order to characterize chromosome phenomena I will quote the following facts. In this hall there is Sergei Stepanovich Kanash, who knows that cultivated American and Egyptian cotton with high chromosome numbers are natural polyploids, even amphidiploids. Some time in the historical past two such varieties of cotton, New World and Old World, were naturally crossed. Each had 13 haploid chromosomes; 26 chromosomes were formed. Then, because of some natural action, of course, Trofim Denisovich, owing to the influence of environment, the chromosome sets were doubled; 52 chromosome varieties of cotton were formed, which, subjected to artificial selection, proved to be excellent material for breeding.

Everybody well able to handle a microscope, every cytologist who studies the meristemic cell of the American and Egyptian varieties of cotton, can see in the cell a double set of chromosomes belonging to the initial partners. Lately, our Soviet cytologist Elenhorn invented a method of staining chromosomes based on the difference in the electrical charges of the parent chromosomes—paternal and maternal. Employ-

ing this method, it is possible to see in each dividing cell the chromosomes of the father and the mother, which confirms Mendel's rule about the purity of gametes in a cross. These chromosomes can be seen in cultivated cotton, and it can be proved that one set belongs to the New World partner and the other to the Old World partner.

Thus, the chromosome theory helped us to ascertain the origin of cultivated cotton.

The same applies to cultivated tobacco, which has 24 chromosomes. It has been proved that 12 of these chromosomes belong to the wild variety of Nicotiana sylvestris, while the other set of 12 belongs to the wild variety of Nicotiana Rusbyi or, what is the same thing, Nicotiana tomentosa.

T. D. Lysenko. Has at least one cytologist proved that it is possible to see either the paternal or the maternal chromosome?

P. M. Zhukovsky. I will leave this rostrum if I am interrupted. There are many such cytologists, those who understand cells.

Knowledge of the chromosome theory enabled us to ascertain the origin of many plants, including the origin of such enigmatic plants as maize. The theory of the origin of maize is now fairly well worked out. As is known, maize is never met with in the wild state now. The former opinion that it originated from the Mexican Euchloena has been refuted. It has been proved that the modern cultivated maize is a hybrid of the bracted maize that once lived in a wild state and representatives of the Tripsacum genus.

A few words about Mendel. Why is the name of this distinguished biologist, at whose grave one should worship, so often derided? We know that our great physiologist, I. P. Pavlov, put up a monument to Mendel outside his Institute in Koltushi. Mendel never studied the theory of evolution. He was a humble researcher whose entire work consists of two short, published essays, one devoted to peas, and the other to hawkweed. In the first essay he showed that there was a certain law in heredity. Many biologists know that this law has been tested tens of thousands of times on self-pollinators. I will quote only one fact. Fairly recently, Wetstein carried out a very interesting experiment. He crossed two different varieties of Funaria mosses. We know that the spores of mosses produce genuine

green Cormophyte plants. Wetstein cut out small pieces of the tissue of the sporagonium of a hybrid and obtained whole plants, i.e., he took tissue from hybrid maternal spore cells and every time obtained the same kind of plants of the first generation of hybrids, resembling each other because there had not yet been a reduction of the chromosomes. He did this under laboratory conditions; the conditions were uniform and he obtained uniform plants. When, however, Wetstein proceeded to cultivate the dyad and tetrad cells, i.e., after the reduction division, he obtained from the spores of each tetrad four plants, all of them manifesting the same type of segregation. Consequently, the divergence during the reduction division proceeded in accordance with Mendel's rules. I am not raising the question as to whether these rules can be disturbed by the influence of external conditions. Trofim Denisovich says they can. I will take his word for it. But I have not seen a single work which explains how hybrids of the first generation should be placed in conditions so that these rules should not operate. This, of course, concerns homozygous self-pollinators.

Michurin did not speak of Mendel in the way it was claimed here. He said that Mendel's rules do not apply to perennials, particularly to fruit-bearing plants. We all subscribe to that, because fruit trees are in themselves hybrids, and in the majority of cases heterozygotes. If fruit-bearing plants were propagated from seeds, "wildings" would result.

Why did Mendel cease his experiments? Because, after his work on peas, he took for his experiments a variety of hawkweed from the Compositae and failed to establish any law. Later it was established that hawkweed has no sexual process and produces apomictic seeds. No law in the Mendel sense could be established. But Mendel's first classical work should be defended.

Anti-Mendelism is a veritable craze at this session, and people go so far as to discredit hybridization in general. The example was quoted of Academician Konstantinov who produced the wheat Melanopus 069 without hybridization, and it was said that Lisitsyn also worked without hybridization. Of course, there are many varieties that have been produced by selection, but an enormous number of varieties of wheat, barley, oats, etc., have been produced by hybridization.

Michurin worked on hybridization. The part hybridization played in the production of frost-hardy and phytophtora-resistant varieties of potatoes is well known. Academician Lysenko himself evolved the theory of selection of parental pairs for hybridization. Nobody denies this, so "don't be more royal than the king." (*Slight applause.*)

What is Michurin's celebrated method of using pollen mixture based on? We are beginning to understand what it is based on. Lately, I have been working in the sphere of embryology. We have made remarkable discoveries. We have found the explanation of the fact that pollen mixture has such enormous theoretical and practical significance. Selection in fertilization does really exist, and we have discovered the biological nature of this selection. A report of our work will soon be published in No. 4 of *Uspekhi Sovremyonnoi Biologii*. Of course, you will find nothing in that report that can serve as grounds for accusing me of being a Mendelist-Morganist. (*Commotion.*)

I pass now to the question of altering the nature of plants, of training. On my word of honour, Trofim Denisovich, nobody will deny that it is possible to alter by training, only the methods by which alterations can be obtained are not so simple. You say that you have succeeded in converting hard wheat into soft. Once upon a time, Yudin used to come out with sensational reports, which ended with a puff, about the conversion of the husked barley into the bare-grained barley. All this is training, it is possible, but I will call it mutation, and let Professor Polyakov call me a mutationist. (*Laughter, applause.*) I attach great importance to mutation. I would like to give an order. I admit the possibility of altering nature by the method of training. Now I want to give an order, not to you, of course, Trofim Denisovich, you will be the boss, you have lots of assistants. Send somebody to the tropics—a plant grows there known as the banana, and it bears very tasty fruit. It is a triploid. What a triploid is many geneticists know very well. If one of the copulating gametes has not been reduced, i.e., is a diploid, and the other is a haploid, the zygote will be a triploid, i.e., it will have three sets of chromosomes. One of the sets cannot find a partner. Such plants are distinguished for their extreme sterility. I think that Michurinists, working on Michurin lines, will not deny that triploids are distinguished for their extreme

sterility, feebleness, and slow growth in those cases where one or both parents were triploids. They cannot be used for crossing. Usually they do not produce seeds.

S. S. Perov. What have the genes to do with this?

P. M. Zhukovsky. I am not talking about genes. I am talking about chromosomes. Evidently, you don't understand this, Sergei Stepanovich. (*Laughter.*) You have gone so far in your disrespect for the Mendelist-Morganists, that when you presented me with two of your works you did not address them to "Dear Zhukovsky" but to "Academician Zhukovsky." (*Laughter.*)

S. S. Perov. I addressed them all in that way.

P. M. Zhukovsky. I doubt it.

Academician Lysenko, send one of your assistants to the tropical zone of the globe, and let him there train bananas to produce seeds. You are all fond of bananas, they are valued because they are seedless.

T. D. Lysenko. As President of the Academy I, of course, can accept orders from our agricultural industry and from the Ministry of Agriculture. From members of the Academy I can accept proposals, but not orders.

P. M. Zhukovsky. That was not a happy expression. Well, I'll withdraw it. But you are beginning to interrupt. I should not like that. You reduced Professor Polyakov to such a state that he lost himself. (*Laughter.*)

It was complained here that Sakharov's buckwheat produced few seeds. That is true. It is the distinguishing feature of triploids. Well, train his buckwheat to give the full quantity of seed. Or send Professor A. R. Zhebrak to the Odessa Institute and let him, under Academician Olshansky's direction, try to produce fertile ears of amphidiploids. You say that it is a waste of time to work with colchicine freaks. But what about mules. They are not freaks, they are splendid animals.

T. D. Lysenko. Read the magazine *Yarovizatsia*. The work reported there was conducted in Krasnodar, and without any colchicine, they obtained fertile ears.

P. M. Zhukovsky. I know about Zakharzhevsky's hybrids, but he managed to raise the fertility of Triticum Timofeyevi only to 45%, and no trace is left of the work.

S. S. Perov. We must go to nature, not to the druggist.

P. M. Zhukovsky. I did not interrupt you, Sergei Stepanovich. Professor Polyakov was attacked here for writing a review of Schmalhausen's book, but I must say that Sergei Stepanovich writes reviews on anything and everything—except, perhaps, on books on gynaecology. (*Laughter.*)

Make the mule fertile. As regards vegetative hybrids, I want, first of all, to reach an agreement about terms. It may be said that I am a formalist and stand for terms. Yes, I stand for clear terms. What is a hybrid? A hybrid, as we understand it, is the product of the fusion of two cells. If a vegetative hybrid were indeed the product of the fusion of two cells, it would be a polyploid, because two diploid sets would have merged, and the result would be a polyploid. But to get a polyploid shoot you must have a bud. Who has seen, in grafting, a bud appear from the fusion of two cells? If anybody finds such a shoot, cuts it and plants it, I will say that it is a genuine hybrid.

We see among the lower plants, for example, in the algae Spirogyra, that two filaments run out parallel to each other and between them a conjugation canal is formed through which the protoplast from one cell passes across to the other. The zygote would be a vegetative hybrid, but it turns out that the filaments are always of different sex, and it is therefore a sex hybrid. This is also the case with the mucor fungi.

Academician Lysenko gives an entirely different interpretation of vegetative hybrids, and I would subscribe to this interpretation on the condition that the stock so acts on the scion that a hereditary variation should take place; in my terminology (I regret to have to confess it) it will be mutation.

T. D. Lysenko. When I reply to the debate I will show you scores of plants, the parents of which were vegetative hybrids. Let any man who understands anything at all about hybridization say that they are not hybrids.

P. M. Zhukovsky. Still, Trofim Denisovich, I beg of you to listen. I believe you about those plants, but I will call them mutations. What mutations are, I will tell you in a minute. Again I say that it is based on the chromosome theory. Mutation means a change in the chromosome that produces a genetic effect.

Trofim Denisovich, you never use the term "mutation," you refuse to recognize it. But we do recognize it. And nature supplies the organic world with mutations almost without limit. What causes mutations? On this point, I am entirely with you, Academician Lysenko: environment, external conditions, cause mutations.

You call it training. But that is beside the point. You refuse to admit that these mutations are caused by changes in the chromosome. That is where we disagree. It has gone so far that the very mention of the word "mutation" or "chromosome" frightens many people. A certain author, I have forgotten who it was, described a maiden who blushed at the sight of a roasted capon. (*Laughter.*)

As soon as the word "chromosome" is mentioned some people blush. (*Laughter, animation.*)

A voice. Men!

P. M. Zhukovsky. Our opponents never mention such terms as vitamins, hormones or viruses. I would advise, not you, Trofim Denisovich, your authority is sufficiently high, but your followers to study, for knowledge is light, while ignorance is darkness. (*Laughter, applause.*)

T. D. Lysenko. Do you apply that to yourself?

P. M. Zhukovsky. I am always studying.

T. D. Lysenko. You don't study very hard.

P. M. Zhukovsky. If you knew how I live, you would know that I do study hard, I study every day....

S. S. Perov. You only read books.

P. M. Zhukovsky. Comrade Dmitriyev, Chief of the Agricultural Planning Administration of the State Planning Commission, said here that there should be no schools.

T. D. Lysenko. Quite right!

P. M. Zhukovsky. I don't know whether Academician Mitin is present, I am afraid he has not displayed proper vigilance. Yesterday's issue of the *Literaturnaya Gazeta* contains an article by Academician Urazov entitled: "Take Care of the Schools. . . ." (*Laughter.*) I think we should take care of scientific schools; we have many in the Soviet Union, and we cannot have only one scientific school.

Concerning the speech delivered by Professor Kostryukova. I saw her here for the first time; I have great respect for her

interesting work. It is true that in the second edition of my textbook *Botany* I wrote that sperms are always cells. Here is proof that I do learn new things. The method of pollination among Cycads is as follows: owing to the difference in osmotic pressure in the spermatozoid and in the archegonium, the former is drawn into the latter with exceptionally great force. The entire protoplasm breaks away from the spermatozoid and gets stuck in the mouth of the archegonium. Only the nucleus enters the ovule. In angiosperm plants the same thing is to be found, i.e., sperm devoid of cytoplasm.

Comrade Kostryukova denies the existence of the gene because nobody has seen one. But the existence of viruses was denied for a long time, but now we can see them. A book was published recently by the All-Union Society for the Propagation of Political and Scientific Knowledge, by Professor Sukhov, I think. In this book it is said that viruses are not living things; they propagate without being alive. Their mere presence causes unusual phenomena in the cells of their host. If they are dead bodies, and we know that catalyzers, for example, give rise to complex biological phenomena, why do you think that the gene cannot give rise to complex phenomena? Surely you must know that the latest experiments have shown that the gene is evidently a protein molecule, and every effort is now being made to ascertain the structure of this molecule.

I was told recently that double pollination is denied. I would like to know what Comrade Kostryukova, Navashin's pupil, will do now. One must not be too adaptive (*laughter*), particularly when one bears in mind that this acquired character is transmitted to offspring. (*Laughter.*)

T. D. Lysenko. I assert that Comrade Kostryukova was already a Michurinist when Academician Zhukovsky had not yet heard the word "Michurin," and yet he says that she is adapting herself today.

P. M. Zhukovsky. No, not today. She published a report on her work in the magazine *Yarovizatsia*.

I must say that the pressure—the great, perhaps just, but powerful pressure that is being systematically brought to bear upon the representatives of the other trend will lead to the extinction of volcanoes, and that soon we will see a number of extinct volcanoes. . . .

S. S. Perov. Mud volcanoes.

P. M. Zhukovsky. . . . unless the opportunity is given for free discussion, and a discussion must be arranged, but not here. We must request that this discussion be arranged in another place, and then we will cross rapiers. (*Laughter.*) As it is, things have reached such a pass that the university is proclaimed as a hotbed of reaction.

T. D. Lysenko. In the field of biology.

P. M. Zhukovsky. Concerning my work on the Experts' Commission and the reproaches Trofim Denisovich hurled at me. People make mistakes, Trofim Denisovich. I admit that I underrated N. I. Noujdin's dissertation. Now, he is a member of the Experts' Commission. I apologize to him. His work on this Commission has shown that, in fighting on behalf of the Michurinists, he is skilled in argument and can turn the members of the Commission to their side. Do not think that he has to adapt himself to anybody. He fights honestly for his clients. I can mention several names in whose cases I played no small role and helped those comrades. Glushchenko, Turbin, and others who are present here, will not deny that I was unbiassed towards them.

But that is not the trouble. Among the dissertations that are entered for the M. Sc. degree there are some, the authors of which use the name of Michurin only as a cloak; the work is of a low level, but the applicants think that if they mention the word "Michurin" the Experts' Commission must grant them their degree. Do not think that work on the Commission is easy. We are responsible for the fact that textbooks are published which say nothing about the achievements of our own Soviet scientists. We are now reorganizing this work, and I have no doubt that we will be able to cope with this task.

I want to make a personal request to Trofim Denisovich. Trofim Denisovich, instruct your organization to issue a comprehensive manual on how to train plants, on how to alter them. Teach us; we too want to learn, and if your methods prove effective, we will accept them. I want agreement. We are just as human as your pupils. You are wrong in sticking labels on us. Trofim Denisovich, this year I lectured to your son. Ask him whether my lectures have spoilt him.

T. D. Lysenko. There is no need to go into family matters.

That is my business as a father. Tell us something that is more important. You complain of being ill-treated. But have you forgotten, Pyotr Mikhailovich, what names I was called in your presence? Did you protest then?

P. M. Zhukovsky. You were never abused in public, at meetings.

T. D. Lysenko. In holes and corners.

P. M. Zhukovsky. That was the gossips. For all that, I want to call for unity. I am one of those who want to work in harmony and not in enmity. We are all Soviet citizens, and we are all patriots. Some of us went personally and others sent their sons to the front. We all fought for our country, and should we really allow things to reach such a pass that people refuse to greet Professor Zhukovsky when they meet him?

Permit me to answer a question: name those who spoke in opposition to hybridization at this session.

If I am not mistaken, it was a comrade from the Mordovian Experimental Station. He said that such and such varieties had been attained without hybridization. (*Applause.*)

Academician P. P. Lobanov. Professor Zhebrak has the floor.

Professor A. R. Zhebrak. Comrades, since at this session there seemed to be some interest in facts dealing with experimental polyploidy and amphidiploidy, I decided to call your attention to some of the data which the group of workers of my department obtained.

We began work on experimental polyploidy of cultivated plants, basing our methods on the data that contemporary science affords.

As you know, the genus Triticum has a polyploid series of 14, 28, and 42 chromosomes. The most widely known representative of the 42-chromosome class, Triticum vulgare, has a broadest geographical range. The winter-hardiest varieties of wheat are met only in the 42-chromosome group, whilst the 14- and 28-chromosome species are much less adapted in this respect.

I am therefore inclined to think that this greater adaptiveness to the environment is the result of an increased chromo-

some number. The greater rapidity with which the soft wheats had evolved as well as their greater adaptiveness to external conditions is due solely to their increased chromosome number. In this species winter-hardy varieties appeared that were not to be found among species with smaller chromosome numbers.

It is on such theoretical premises that I set forth to extend the polyploid series in the genus Triticum beyond the 42-chromosome group.

I thought it would be of some theoretical interest to elucidate the origin of the 42-chromosome wheats, a question that attracted some attention in biology. The wild ancestors of the 28- and 14-chromosome wheats are known, whereas for the 42-chromosome wheats they have not been found. Academician V. L. Komarov in his *Origin of Cultivated Plants* writes that the 42-chromosome wheats are the result of hybridization.

We set forth to obtain a 42-chromosome type of wheat by the experimental polyploidy method and by the hybridization of remote forms. With this in mind we crossed two species of wheat whose chromosome numbers differed, e.g., Triticum durum and Triticum monococcum. The first species has a set of 28 chromosomes in its somatic cells, whilst the second has 14 chromosomes. The first generation hybrids had 21 chromosomes and were absolutely sterile: not a single seed developed on the control plants. By applying weak colchicine solutions to dry seeds and seedlings we were able to restore fertility to these sterile hybrids; thus was a 42-chromosome type of wheat synthetically obtained.

But our task was to obtain a variety with a still greater chromosome number. We therefore proceeded to cross a 28-chromosome species with another 28-chromosome species described by Zhukovsky, known as Triticum Timofeyevi. This latter was chosen because it is a genetically isolated form, i.e., when crossed with other 28-chromosome species it produces highly sterile F_1 hybrids that give but few seeds on some plants, whereas the majority are altogether fruitless.

Here I shall demonstrate only one representative of a 28-chromosome species crossed with Triticum Timofeyevi. Here is a spike of Triticum Timofeyevi. The spike of the hybrid plant is narrow. If however the hybrid plant be treated at the various stages of its development with a weak solution of col-

chicine the chromosome number may be doubled in many parts of the seedling, and if spikes develop from such a section they are normally fertile. In the following generations normally fertile and stable plants develop from the seeds of such ears. This method enabled us to produce experimentally a variety of wheat with a new chromosome complement.

Here is a representative of the new chromosome group of wheat. This variety has 56 chromosomes in its somatic cells. We obtained this 56-chromosome variety by crossing Triticum Timofeyevi with 19 varieties of Triticum durum, with 6 varieties of Triticum turgidum, 3 varieties of Triticum polonicum and with 1 variety of Triticum orientale.

Thus if all the varieties of the 28-chromosome species be taken into account, this 56-chromosome type is actually an amphidiploid obtained from the cross of Triticum Timofeyevi with 32 varieties of the 28-chromosome group of wheats.

We consider these new types as a new species of wheat since they differ from all hitherto known forms of wheat both morphologically and by their chromosome number.

Our next step in the production of new wheat groups was to create a 70-chromosome form of wheat. This we obtained by crossing Triticum Timofeyevi with Triticum vulgare and Triticum durum with Triticum vulgare, with a subsequent doubling of the chromosome sets of F_1 hybrids.

It therefore follows that experimentally by hybridizing remote forms and by means of chemical treatment with agents that exert a specific influence on nuclear material we obtained new types of wheat hitherto unknown in nature.

Another question that bears some relation to the present controversies is that which deals with the recovery of fertility in remote hybrids. Is this caused by a specific action on the chromosome complement of colchicine which at the same time restores fertility? Or is the restoration of fertility caused by the doubling of the chromosome number and the recovery of paired chromosomes?

To elucidate this point we carried out the following experiment. The data that I am going to touch upon were jointly published by myself and my collaborator Afanasyeva (a Master of Biological Sciences) in the *Doklady of the Academy of Sciences of the U.S.S.R.* in 1948. (Vol. 59, No. 6.)

In our experiments the doubling of the chromosomes and the very process of hybridization itself were dissociated in time. Formerly we obtained our wheat amphidiploids in the following way: first we would cross Triticum durum and Triticum Timofeyevi, obtaining F_1 hybrids whose chromosome number we then proceeded to double, thus creating normal, fertile and non-segregating plants.

We likewise attempted to obtain polyploids in pure stock lines of Triticum durum and Triticum Timofeyevi. Such forms were actually obtained both in the one and in the other. However, the doubling of the chromosome number in pure 28-chromosome species gave 56-chromosome autopolyploids that proved to be highly sterile.

The low fertility of autopolyploids from the point of view of contemporary genetics may be explained by the fact that cell structures with several homologous chromosomes arise which during the process of meiosis form polyvalent groups; as a result their disjunction is impeded and an insufficient number of viable gametes is produced.

This was confirmed in experiments with Triticum durum and Triticum Timofeyevi polyploids. When we crossed these 56-chromosome autotetraploids we obtained amphidiploids by a new method. If formerly amphidiploids were obtained by producing at first a sterile F_1 hybrid and then doubling its chromosome set, by the second method we proceeded at first to double the chromosome number of the parent species and then cross their autotetraploids, obtaining in this manner a fertile amphidiploid without any further application of colchicine.

We therefore came to the conclusion that the recovery of fertility in remote hybrids is the result of the doubling of the chromosome set and the restoration of paired chromosomes.

I am of the opinion that these data on experimental polyploidy in cultivated and wild plants are a valid proof of the soundness of the contemporary chromosome theory of heredity.

Naturally, our aim was not only to create new types in respect to chromosome numbers and to the sum of morphological characters. We undertook these experiments in order to create new wheat varieties of practical value. What have we obtained?

We have obtained comparatively vigorous plants with good

spikes and very large grains. One thousand of our amphidiploid seeds weigh 80 g. and over. The spike of the amphidiploids in respect to brittleness is intermediate between the brittle spike of Triticum Timofeyevi and the non-brittle spike of Triticum durum. Although the weight of the amphidiploid grains is considerable, they are not easily threshed.

We realized that our material demanded a great deal of hard and patient work before we could obtain results of practical value. We began to employ two methods: the method of selection of the amphidiploids and the method of subsequent hybridization of the newly obtained polyploids with the normal Triticum vulgare.

A voice. And their yielding capacity?

A. R. Zhebrak. I shall touch upon these questions in due order.

On crossing our 56-chromosome wheats with a 42-chromosome variety 49-chromosome trihaploids were obtained.

These hybrids carried the haploid sets of three distinct species. The hybrids grow well, have long stalks, but are usually sterile; the spikes of the first generation have but few seeds.

But we knew that this 49-chromosome variety would give rise to a polyploid series beginning with 14 chromosomes and ending with 98. This was foretold on the basis of purely theoretical calculations involving a knowledge of the nature of genomes. These F_1 hybrids were obtained only in 1942. We proceeded to intercross them hoping that in the process of segregation types would appear varying in fertility and other characters.

As a result of the propagation of these 49-chromosome forms some extremely primitive types of wheat plants were obtained. At the same time perfectly cultivated types appeared that had a high yielding capacity. Finally, an exceptionally great variety of spike characters was obtained. Both loose- and compact-spiked forms appeared. We likewise obtained a branched type of wheat although there were no branched forms in our collection.

Permit me to put a question to Comrade Yakubtsiner who is present here. Would you please tell me, to what species these wheat specimens belong? (*Hands a few specimens of wheat to Yakubtsiner. Yakubtsiner proceeds to examine them.*)

We are quite certain that they belong to Triticum compactum. I am not trying to puzzle you. I just want to decide this question as was done when I invited P. M. Zhukovsky who unhesitatingly declared that they belong to Triticum compactum. I likewise invited Comrades Kuleshov and Kuznetsov and they confirmed this opinion. I therefore came to the conclusion that this really must be so, but mind you, this is the fifth generation of the trihaploid hybrid.

T. D. Lysenko. What you are telling us is quite a common matter. Come to Gorki and you will see it all there.

A. R. Zhebrak. This type is of no interest at all, it being a trihaploid and sterile. A combination of three species produced Triticum compactum, and there is no doubt about that.

Now I shall touch upon the principal question. Naturally we are interested in characters that are of practical value. The character of highest economic value in any cultivated plant grown for its seeds is high fertility.

The first generation had an extremely low fertility. In the second generation some plants appeared that gave as much as 250 grains. In F_3 the fertility was somewhat raised. In the fourth generation the maximum number of grains per plant rose to 625, and this summer we obtained a fifth generation that gave over 1,500 grains per plant.

This is for the information of those who were curious about the fertility of polyploid types of wheat.

We possess at present 96 families of the fifth generation, that are perfectly even, well-grained and possess a number of other good qualities. Many of their qualities are very promising. Some of the plants are curiously branched and their spikelets have 9-10 and even 11 grains. Plants with 6-7 grains in a spikelet are quite numerous.

In what way are these facts related to the chromosome theory? Do they in any way follow from the premises of this theory? I have demonstrated that the 49-chromosome hybrids have a low fertility. The increase in fertility is directly proportional to the recovery of paired chromosomes. The majority of highly fertile forms are grouped around the 42- and 56-chromosome classes. Only forms with a balanced number of chromosomes are normally fertile and are of any interest for further selection.

Just one more piece of information and I shall finish with the wheat question. In our work with the wheats we have been using not only methods of experimental induction of polyploidy and remote hybridization, but also the pure-line method, the method of individual selection. I confess that this latter method gave better practical results than experimental polyploidy and remote hybridization, since with these we have only raised the fifth generation. Naturally the fifth generation is a much too recent one to have obtained valuable forms. But all that I have demonstrated here leaves no doubt that such varieties will be of practical significance.

Permit me to refer to the data concerning two of our varieties that were handed over in 1946 to the State Cereal Variety Trial Commission.

Here is the report of the inspector of the State Commission for the Moscow Region under the heading: "The best varieties of winter wheats on the state seed testing plots of the Moscow Region in 1947." The data presented refer to the Dmitrov and Podolsk plots (given in centners per hectare).

	Dmitrov plot	*Podolsk plot*
Moscow 2459 (standard)	39.2	29.8
Moscow 4	37.7	31.5
Wheat-Agropyron hybrid of Academician Tsitsin 599	41.9	27.9
Our variety—Lutescens 269a	41.9	29.2
Albidum 05	41.1	31.8

On the Dmitrov plot our wheat gave the same results as the wheat-Agropyron hybrids of Tsitsin, whereas in Podolsk they gave somewhat better results.

These forms were produced by the usual methods of classical genetics and selection.

At the present time some of the polyploid hybrids yield as much as 1,500 seeds per plant and about 108 grains to a spike. The variety that we handed over to the State Cereal Variety Trial Commission and which showed up fairly well is of a much poorer quality than our polyploid types.

Voice from the audience. How many centners to a hectare does the polyploid yield?

A. R. Zhebrak. I said that our polyploids have been bred only up to the fifth generation. (*Commotion in the hall.*) We have 96 forms; about fifty of them are promising inasmuch as they excel our standard types.

S. S. Perov. In how many years do you intend to hand them over?

A. R. Zhebrak. Next year we intend to hand over to the State Cereal Variety Trial Commission the material concerning several of our types. This year we shall reproduce them.

T. D. Lysenko. You say that this year you intend to reproduce them; but the year is almost over!

A. R. Zhebrak. The year is ending for the harvesting of winter crops, but it isn't over for the sowing of winter crops. We intend to sow them to increase our stock, so that in 1949 we shall have enough material to hand over for a state approbation of polyploid forms.

Permit me to touch upon some more data concerning buckwheat and millet polyploids. In our laboratory A. N. Lutkov began work on the induction of polyploids in buckwheat in 1941. During the war this work was stopped, and we were able to continue it only after the war. At present we have obtained polyploids for a number of varieties of buckwheat. We chiefly worked with the Byelorussian early-ripening scantily seeded types of buckwheat. For these early ripening varieties we obtained tetraploid forms whose seeds are usually heavier than those of the diploids by 10-15 g. per 1,000 seeds. The usual diploid form weighs from 18-20 g. per 1,000 seeds, whereas our tetraploids weigh as much as 25 g.

It should be pointed out that the fertility of buckwheat tetraploids is somewhat lower than that of ordinary diploids. So as to overcome this low fertility we have resorted to Michurin's method of crossing remote geographical races. We have tetraploids of the Byelorussian varieties, of the Buryat-Mongolian forms, of the Altai buckwheats, the Kamishin ones and of the Bogatyr variety. These tetraploid forms, each of a low fertility, are being intercrossed so as to overcome this shortcoming.

These crossings are theoretically based upon Michurin's teaching about the hybridization of remote geographical races. We have been intercrossing the autotetraploids of buckwheat

and millet with the aim of increasing their fertility just as we have done with the hybrids of Triticum durum and Triticum Timofeyevi.

Hybrid variability of the tetraploids may be expressed by the following mathematical formula. If the hybrid variability in the diploids is equal to 2^{16}, in the tetraploids it will be equal to 2^{32}. This exceeds the usual hybrid variability of the diploid about 28,000 times. That is why the hybridization of tetraploids is to be highly recommended as a means of overcoming the main obstacle, namely, their low fertility and increasing variability due to recombination.

Similar experiments are being carried out with millet tetraploids. Last year we obtained tetraploids for six different varieties of millet grown in Byelorussia. At present we have at our disposal a tetraploid-millet variety produced by Comrade Dynnikov at the Uralsk Selection Station. It should be noted that Dynnikov's tetraploid is particularly interesting, because it arose spontaneously under normal conditions. Dynnikov found his tetraploid among normal crops. Like tetraploid buckwheat the millet tetraploid likewise possess larger seeds. However, the tetraploid millets are less fertile than the diploids.

Comrade Afanasyeva and I published a paper on this subject in the latest issue of the *Doklady of the Academy of Sciences.* (Vol. 61, No. 3.) I refer all those who are interested in the figures to this paper.

This year we likewise began intercrossing tetraploid millet varieties in the hope that their fertility might be increased.

In my opinion experiments with amphidiploids, and with tetraploids of buckwheat, millet, rye and a number of other cultivated plants are the most original in the Soviet Union. Their value is not only of a practical kind, it is of considerable theoretical interest as well. In the experimental induction of polyploids in cultivated plants it is important to note that they were obtained by the action on the hereditary material of such an external agent as colchicine. This completely shatters the theoretical premises of autogeneticists who claim that the germ plasm is isolated from external factors and stable in respect to external influences.

Experiments on induced polyploidy in cultivated plants demonstrate that external factors such as colchicine, acenaphthene,

temperature and others exert a specific influence on the hereditary material, on the chromosome complement of the cell.

Experiments with polyploids confirm the dialectical interdependence between the nature of the plant and its environment; they likewise demonstrate that the hereditary nature of a plant which is dependent upon its nuclear complement may be controlled by man. These experiments better than any other work in the field of genetics confirm the justness of the aphorism of Marx, that up to the present philosophers have been trying to interpret the world, whereas the problem is to change it.

Contemporary experimental genetics has mastered the means of remodelling the hereditary basis of plants and reconstructing the vegetable world.

Voice from the audience. What has experimental genetics given to industry?

A. R. Zhebrak. In my opinion these investigations raise the level of our Soviet science. They are in unison with Comrade Stalin's instruction that the task of Soviet scientists is not only to overtake the achievements of science of other countries, but to outstrip them.

A voice. What science?

A. R. Zhebrak. The group of investigators that are working under my lead will do its utmost in the field of experimental polyploidy to fulfil the instruction of our great leader and teacher. (*Applause.*)

Academician P. P. Lobanov. I call upon Professor N. V. Turbin, Head of the Department of Plant Genetics of the Leningrad State University.

Professor N. V. Turbin. The critical state of present-day Morganist genetics is most sharply and vividly reflected in works like the article by Professor Dubinin that has been repeatedly mentioned here.

Such works are characteristic of the most fanatical and orthodox adherents of Mendelism-Morganism who, with inflexible obstinacy and in the face of obvious facts, uphold even all those things in Mendelism-Morganism which the more farsighted representatives of this obsolescent theory are gradually discarding.

But the critical state of Morganist genetics, its incapacity for further progressive development, is also reflected in the appearance of those new views among the adherents of this theory, on the basis of which Comrade Alikhanian tried to argue that the alleged new conception of the gene had nothing in common with the conception that has been criticized by the chief speaker at this session and by speakers who have followed him.

In this connection, I want to deal with the question as to how far these new trends, these new views concerning the nature of the gene, really eliminate the metaphysical and abiological character of the basic concept of this theory. I have in mind those new trends and new views expressed by certain followers of Morganism on the question of the gene and the nature of its functions that are connected with the solution of such problems as the relation between the gene and the enzyme which the gene produces, between the gene and virus, between the gene and protein-antigene, between the gene and plasmogene. All these new trends in cytogenetic thought that have made their appearance lately have, undoubtedly, a shamefacedly-expressed tendency to gradually obliterate the difference between the chromosome genes, as special units of hereditary substance, and the somatic elements of protoplasm. But the significance of these ideas must not be overrated, as is done, for example, by the English cytogeneticist Fyfe, who has attempted to analyze the present state of genetics and the significance of the work of Academician Lysenko.

Fyfe arrived at the conclusion that the new ideas are leading to the modification of the fundamental propositions of the theory of the gene in the same direction as that towards which Lysenko is trying to do, although he says that Lysenko is approaching it from an entirely different experimental point of view.

This opinion is wrong, however. The new facts which are today knocking a breach in the metaphysical views regarding heredity, and are compelling cytogeneticists to revise some of the premises of their theory, are not properly explained by them, because the minds of cytogeneticists are still dominated by the false, metaphysical idea of a hereditary substance and by the Weismannist principle of the continuity of plasm.

The appearance of these new ideas, of the new hypotheses of Morganist genetics, is in itself striking evidence that some of the prominent adherents of this theory of genetics, who up till now have ignored the facts obtained by Michurinists and which undermine their theory, are themselves coming up against such facts more and more often.

But they are incapable of breaking away from the fundamental pseudoscientific dogma of Mendelist-Morganist genetics, from the theory of a hereditary substance; they are incapable of drawing correct conclusions from these facts. These scientists are trying to save the bankrupt metaphysical dogma about a hereditary substance by means of various supplementary hypotheses.

From the point of view of the progress of science, these supplementary theses do not mark a step forward; on the contrary, they can only retard the development of modern genetics, for they prevent one from looking at facts openly and from drawing unprejudiced conclusions. It must be admitted, therefore, that, in spite of the discovery of new facts which objectively are valuable from the scientific-gnosiological point of view, present-day Mendelist-Morganist genetics are incapable of progressive development. Inglorious in the present, it has no future. The future belongs to the new Michurin genetics.

The only thing that can prompt some adherents of Morganism to assert that they are opponents of Weismannism, in spite of the fact that they recognize the existence of genes as units of a special hereditary substance, is their failure to understand the essence of Weismannism and the fundamental thing that divides the Weismann trend in the study of heredity from the Michurin trend.

From this rostrum Docent Alikhanian declared that he has fought and will continue to fight Weismannism. But what does he mean by Weismannism? Up till now, most biologists have been rightly of the opinion that Weismannism is the theory that divides the living body into two plasms—continuous and independent germ-plasm, and somatic plasm, a theory that inevitably leads to the deduction that the inheritance of acquired characters is, in principle, impossible. What is Docent Alikhanian's attitude towards this essence of Weismannism, towards

the principle of the continuity of the germ-plasm and towards the deduction that the inheritance of acquired characters is impossible? Judging by his attitude towards these latest amendments, to the fundamental concept of Mendelism-Morganism, whose mission it is to save the basis of Morganism, namely, the principle of the continuity of the germ-plasm and the deduction that the inheritance of acquired characters is impossible, Docent Alikhanian, like all the other consistent Morganists, is not an opponent but an adherent of this latest form of Weismannism.

The new Michurin theory of heredity did not arise in our country by chance, it was not the result of the failure of its adherents to understand the chromosome theory of heredity, as we often hear the opponents of Michurin genetics say. This theory arose in the regular order of things as a result of the higher demands presented to agrobiology by our socialist agriculture and of the creation of conditions which facilitated both the presentation and the correct solution of the big, new scientific problems connected with the search for means of directing the development and heredity of agricultural plants and animals. It arose as a result of the thorough tests to which previously known facts had been subjected, and of the discovery of new ones which proved that the explanation of heredity given by the chromosome theory is useless.

It is, therefore, no accident that the criticism that we Michurinists have heard from the representatives of Mendelist-Morganist genetics has one common feature, namely, nothing is said about the facts obtained by Michurin genetics.

The opponents of Michurin genetics try to make it appear that Michurin genetics is merely a new speculative concept unsupported by experimental facts, whereas, they claim, Mendelist-Morganist genetics is based on experimental data. This is an utter distortion of the actual situation.

We can say that at the present time Michurin genetics is in possession not of single, isolated facts, not of single links torn from the complex chain of interconnected phenomena of heredity and giving no complete picture, but of a whole system of inherently connected facts which undermine Mendelist-Morganist genetics and constitute a firm foundation for Michurin genetics.

In this connection, I would like to remind Docent Alikhanian, and the other opponents of Academician Lysenko, of the main facts which, in our opinion, completely undermine the theory of the gene. First of all, there are the facts about vegetative hybridization, which show that it is possible to obtain hybrid organisms that combine the characters of the initial varieties used for grafting without combining the chromosome sets of these initial varieties and, consequently, without combining the hypothetical genes located in the chromosome pairs.

Michurin genetics attaches special importance to this category of facts, not because Academician Lysenko regards vegetative hybridization as the chief method of plant breeding, as some claim, but because vegetative hybridization is the chief and most striking proof of the unsoundness of the theory of the gene. These facts alone should be sufficient to induce one utterly to reject the theory of the gene as a fallacy.

If the opponents of Michurin genetics want seriously to discuss this problem, they ought to try to explain these facts about vegetative hybridization or disprove them, to prove that they are not authentic. But it is not so easy to disprove facts. Facts are stubborn things. I will not deal at length with the facts about vegetative hybridization; I will merely quote an example which, I think, should be especially appreciated by the Mendelist-Morganists, because it is connected with the methodics employed by Winkler and Krenke to produce what is called "grafting chimeras."

We know very well that the adherents of Mendelism-Morganism place a number of known examples of vegetative hybrids in the category of so-called grafting chimeras. In their opinion, the chief difference between grafting chimeras and genuine vegetative hybrids is that grafting chimeras are the mechanical uniting of tissues, the growing together of the components of the graft, and, therefore, produce an identical seed generation which reproduces the characters and qualities of that component of the graft to which the sub-epidermal stratum of the cells in the cone of growth belongs. But now we know of a case where so-called grafting chimeras, obtained strictly in accordance with Winkler's method, between a tomato and nightshade, produced a seed generation, some of the plants of which bear characters of the chimera structure; one and

the same organs bear characters both of the nightshade and of the tomato.

Try to explain this fact on the basis of the theory of the gene.

P. M. Zhukovsky. Those facts are known.

N. V. Turbin. If such facts are known to you, Pyotr Mikhailovich, then I would ask you, as an older comrade, to explain them to me on the basis of the theory of the gene.

I specialize in genetics and I am sufficiently familiar with the theory of the gene, but I would not undertake to explain this fact on the basis of the theory of the gene. And I am afraid that you, too, Pyotr Mikhailovich, will be unable to do so.

A second group of facts shows that the segregation of parental characters that are combined in the hybrid organism can take place not only in its sex generation as a result of reduction division, but also in the vegetative generation, in the absence of disjunction and recombination of chromosome pairs.

As chairman of the Experts' Commission, you, Pyotr Mikhailovich, are familiar with the dissertation I wrote for my Doctor's degree on the problem of the vegetative segregation of plant hybrids. You have never challenged the facts quoted in that dissertation and the deductions drawn from them; on the contrary, you passed a favourable opinion on my work. But the essence of that dissertation is that the facts about vegetative segregation quoted in it undoubtedly prove that there is no connection between the segregation of characters—the phenomenon on which the whole of Mendelism and subsequent Morganism were built up—and the segregation of the chromosome pairs. Please explain to me this category of facts too with the aid of the chromosome theory of heredity.

The next group of facts shows that, under certain conditions, organisms known to be heterozygous in nature, of hybrid origin, produce a non-segregating generation, and that under other conditions, organisms that are of the homozygous genotype, of "pure" origin, can produce a segregating generation.

Further, we know of facts which show that changes in the phenotype of hybrids caused by training under definite conditions of existence, responsively affect the segregation of the sexual generation. If the development of the characters of the hybrids tends in the direction of the mother, their progeny

shows a larger number of individuals possessing the corresponding characters of the mother.

If the same kind of hybrids possessing the same initial genotype are diverted by training to the side of the paternal organism, the relative number of progeny with paternal characters increases accordingly. According to the theory of the gene, however, each pair of genes must produce an equal number of gametes bearing the paternal gene and the maternal gene, irrespective of whether the maternal or paternal gene exercises predominating influence upon the development of the corresponding characters. Thus, according to the theory of the gene, changes in the phenotype caused by the influence of conditions in which it is trained should not exercise a responsive influence on its genotype and on the segregating generation. Time will not permit me to quote facts of this kind that I and my assistants have obtained in experiments with tomatoes. Academician D. A. Dolgushin quoted similar facts when he spoke. And such facts are also quoted in some of the works of formal geneticists (Bateson, Crane, Correns, and others).

Lately, an entirely new and not yet adequately explained category of facts have come to light connected with the new conception of the nature of the process of fertilization proposed by Academician T. D. Lysenko.

As a result of four years of experiments with tomatoes, we succeeded in experimentally obtaining tomatoes which obviously and unquestionably combine the characters of two paternal varieties—copollinators. A cytological investigation of these plants showed that they are ordinary diploids.

After studying the progeny of these unusual hybrids we obtained flawless proof that they sprang from egg cells which had experienced the fertilizing influence of two paternal pollinator varieties.

The time has come to revise the commonly accepted view concerning the fertilization process, according to which it amounts to the fusion of the male and female nuclei of two sex cells. We now have reasons for doubting the correctness of this view. Pyotr Mikhailovich, I want to inform you, since you are an embryologist, that we are now making a detailed cyto-embryological investigation of this phenomenon

with the view to ascertaining the way in which several paternal elements influence the fertilized egg cell.

Similar data have been obtained by Salyamov, Feiginson and Avakian, Babajanyan, Ter-Ovanesyan and others. Try to link these facts in some way with the gene theory of heredity.

Many speakers here have already quoted facts obtained in experiments to transform winter plants into spring plants and vice versa. These facts show that it is possible by training plants under definite conditions of existence to obtain directed variations not only in their body characters, but also in their hereditary basis, if the nature of these characters have been sufficiently studied; that is to say, experiments have proved that acquired characters are hereditary.

In this connection I want to emphasize the difference in principle between the very approach of Michurin and Morganist genetics respectively to the conception of the way hereditary variations first arise. According to Morganism, the gene, being a protein macromolecule, possesses a definite stability determined by the intramolecular bonds of the atoms and atomic groupings that constitute the molecule. The action of a source of energy that exceeds the energy of the inner bonds of the gene molecule may cause some groups of atoms to break away from the molecule, or cause a certain intramolecular regrouping. How such changes in the gene molecule will affect the corresponding character, or characters, obtained from the altered reproductive cell, in principle, cannot be predicted; and it is impossible deliberately to induce a repetition of this mutation by the action of the same factor because, being based on this kind of uncontrollable reconstruction of the gene molecule, mutation cannot be directed, its quality is independent of the nature of the operating factor and of the physiological processes in the organism. Therefore, according to the theory of the gene, it is impossible to obtain directed mutations.

According to Michurin genetics, the quality of hereditary variations is naturally connected with the physiological reactions and changes in the organism in the course of its development caused by the action of external conditions. Variations in the characters of living organisms that condition organic evolution and the process of selection are not haphazard, with no orderly connection with the nature of external conditions;

on the contrary, these variations are obedient to order and are always responsive to the nature of the external factors which cause their appearance. It is only because of this orderly connection between hereditary variations and the nature of the external conditions which act upon the organism that variations of characters are observed, continuing in one and the same direction through a number of successive generations of the organism. Thus, we have confirmation of the correctness of Darwin's view that characters which had begun to change in a given direction in ancestors, continue to change in the same direction in the progeny, if the progeny are subject to the action of the same conditions that caused the original change in the ancestors. It is precisely this orderly continuation of variation that serves as the basis of the creative role of selection. Without it, selection is inconceivable. Without it, selection simply becomes a mechanical sieve which sifts already existing varieties, but creates no new ones possessing new qualities, new characters, which were absent in the original material.

Such are the main facts that undermine Morganist genetics and serve as a firm foundation for Michurin genetics. Behind these facts are real, live people, our Soviet biologists and agrobiologists who did not blindly worship the "world" authority of Morganism, and who actually facilitated the progressive development of Soviet genetics.

No investigator of heredity who is prompted by the desire to seek the truth, and is capable of overcoming the inertia of habitual though obsolescent views, can pass by these facts with dull indifference.

Instead of treating the factual material on which Michurin genetics is based, material obtained as a result of the enormous, painstaking and thorough efforts of numerous investigators, in a businesslike and scientific manner, the opponents of Michurin genetics who have spoken at this session preferred to say nothing about these data which are not to their liking. True, silence is also criticism of a sort—the most stupid sort of criticism.

I will deal with some of the arguments advanced by some comrades in defence of the theory of the gene and as criticism of the Michurin theory of heredity.

Doctor Rapoport stated in his speech that none other than

Darwin was the founder of the theory of the gene. His object in making that statement was to show that the theory of the gene was not advanced by avowed anti-Darwinists like Bateson, de Vries, Johannsen and others, who, up till now, have been regarded as the creators of this theory, but that it is the beloved offspring of Charles Darwin himself.

This astonishingly bald statement simply turns to dust at the very first comparison between Darwin's pan-genesis hypothesis and the theory of the gene.

Darwin recognized the transmission of acquired characters and vegetative hybridization, whereas the theory of the gene denies the reality of these phenomena and explains the mechanism of heredity in a way that makes these phenomena impossible in principle.

The central idea in Darwin's hypothesis is that the germ cell is the product of the development of the whole organism. According to this view, all parts of the body of the developing organism take part in the formation of the germ cell through the transmission of special buds—gemmules, and every part of the body, as it were, reproduces itself in the offspring.

Thus, on the main approach to the interrelations of the body and the germ cells, Darwin's theory is the very opposite of the germ-plasm theory upon which the theory of the gene is based. And although Darwin's conception of the gemmule, like the conception of the gene, is metaphysical, the two conceptions are not identical in their main features.

Hence, Comrade Rapoport's naive attempt to present Darwin as the founder of a theory which logically and historically runs counter to the whole spirit of Darwin's views on heredity, variation and selection, his attempt to present Darwin as the founder of the anti-Darwinist theory of the gene, is absolutely groundless and can mislead nobody.

Comrade Rapoport said in his speech that the denial of the existence of the gene is an idealistic mistake committed by the adherents of Michurin genetics. Comrade Alikhanian, in the speech he delivered today, also supported this criticism when he said that by denying the existence of the genes we are slipping into the idealistic position. But is that the case?

The logic of statements of that kind is as follows.

Genes are material. If they do not exist, then the phenom-

enon of heredity has no material basis. As if the repudiation of the assumption that genes exist in the germ cell, the material link between parents and offspring, means denying the existence of the germ cell itself as the real material basis of heredity! This kind of argument is, to say the least, cheap sophistry. As regards the present position and future prospects of the scientific trends that are connected with the study of the structure, biochemistry and physiology of the cell, I think they have nothing to lose but everything to gain by developing under the influence of the guiding principles of Michurin genetics.

In the endeavour to prove the practical usefulness of Morganist-Mendelist genetics, Comrade Rapoport ascribes to this genetics the discovery of heterosis and the elaboration of the method of improving seeds based on heterosis. But this is contrary to the facts. Does not Rapoport know that the increased vigour and productivity of hybrids was revealed long before the appearance of Mendelism and theoretically explained by Darwin? All that the Morganists can claim authorship of is the term heterosis, but the invention of a term is not the discovery and still less an explanation of the phenomenon to which the term is applied. It may be said that the Morganists have proposed a particular method of obtaining heterosis seeds by crossing previously obtained inbred lines, but this method is in no way superior to the crossing of different varieties, different reproductions of the same variety, so that the long and complicated process of previously obtaining the inbred lines proposed by the Morganists is absolutely superfluous.

A few words about Comrade Rapoport's interjection about cytogeneticists being aware of useful mutations and their ability to produce them artificially. I do not know what facts he is referring to. I can only recall that in summing up the entire literature on this subject, Academician Schmalhausen arrived at the conclusion that useful adaptive mutations are unknown. Proceeding from this, he built up the theory of stabilizing selection, which is supposed to explain how organic evolution, possessing features of the adaptive process, can take place in the absence of adaptive mutations in nature. We can speak of useful mutations only in one sense, namely, that these mutations are useful to those who are studying them, because, while the mutations that cytogeneticists are studying do not

and cannot serve as a source of material for organic evolution, they are quite a reliable source of material for writing dissertations and for the relatively easy receipt of scientific degrees.

J. A. Rapoport. It is a better theory than yours. Obscurantists!

N. V. Turbin. Wishing to sting the Michurinists, Comrade Rapoport said that we must train honest cadres who will look facts straight in the face and not lie to themselves or to others. But the methods to which Comrade Rapoport resorts in defending the theory of the gene—hushing up and shying at facts, distorting well-known facts, offensive interjections and hysterical outcries—all indicate that Comrade Rapoport himself does not belong to the honest cadres.

In conclusion I want to say that as a researcher and college teacher I entirely associate myself with the opinion of those comrades who regard as intolerable the present situation as regards the teaching and study of genetics in which we see the predominance of the adherents of Morganist-Mendelist genetics in our colleges and universities, in scientific research institutes and on the editorial staffs of periodical and non-periodical publications.

This situation must be radically changed. The themes of the researches in genetics conducted by biologists and scientific institutes must be revised, and the institutes must be freed from the domination of the fanatical adherents of Mendelism-Morganism, from persons who under cover of their high scientific titles often engage in what is in fact digging holes and filling them again.

The departments of Plant and Animal Breeding and of Darwinism in our colleges and universities must be reorganized on new lines, their attention must be diverted from the study of various pseudoscientific problems based on Mendelism-Morganism to the urgent problems of progressive Michurin genetics; and they must be reinforced with scientific cadres who have actually proved their ability to study fruitfully the problems of Michurin genetics and Darwinism. We have these cadres, but as a rule they have no access to chairs, and where they have gained access to them, everything is often done to get rid of them.

It is high time to put a stop to the unrestrained propaganda of reactionary Morganism among wide circles of biologists and agrobiologists; the necessary conditions must be created for the development and propaganda of Michurin genetics and Soviet, constructive Darwinism. This is called for by our Soviet life, which is so stern and ruthless towards the stillborn offspring of debilitated metaphysical thought and so benign to the irresistible growth and practical application of constructive scientific thought. (*Prolonged applause.*)

Academician P. P. Lobanov. The session will adjourn until 11 a.m. on August 6.

NINTH SITTING

Morning, August 6, 1948

Academician P. P. Lobanov. The session is resumed, Academician I. I. Schmalhausen has the floor.

Academician I. I. Schmalhausen. I must first of all apologize for having been unable until now to participate in the deliberations of the session on account of my health. To tell the truth, I should not be here and speaking now. But the fact is that I have been the object of so much attention that if I were to remain silent it would probably be misunderstood. I therefore cannot refrain from making some remarks in elucidation of the charges which have been levelled at me.

The first and chief charge is that I believe in autogenesis. It was, moreover, stated that in this respect I am not a continuer of the line of my teacher, Academician Severtsov. Severtsov, presumably, was of a different opinion, since, of all his numerous disciples, he chose me as his successor. He evidently considered that I was the most consistent of his followers.

Actually, I have always endeavoured to stand by the position of the materialist explanation of evolution, and I have consistently fought idealism in all its variations. Attempts have been made here to class me with the geneticists, and, what is more, with the formal geneticists. For the sake of those who are not familiar with the facts, I must say that I am not a geneticist at all, but a morphologist, an embryologist, a phylogeneticist. At most, the only connection I may have with genetics is my work on the phenogenetics of racial characters in domestic fowl. None of my other work has, or ever has had, any connection with genetics, still less has my work any connection with formal genetics.

I have tried to be a consistent materialist, and I think that this is quite clearly reflected in my works. It was from this position that I have criticized all the idealist views I have been charged with here. In the *Problems of Darwinism,* on pages 194-208, you will find a criticism of Weismann, of de Vries, of formal genetics, of the views of Lotsy and of the theory of preadaptation. Many of these theories, the theory of preadaptation, for instance, were subjected to such thoroughgoing criticism in the Soviet Union for the first time by me.

What is the basis for the charge of autogenesis, and hence of idealism?

It apparently rests on the question of the sources of variability. Here is what I say about the sources of variability:

"Darwin assumed that the source of indeterminate hereditary variations was the factors of the external environment. Although the geneticists have usually held by autogenesis, the facts they themselves have found contradict this view.

"Efforts to induce the formation of mutations by the action of external agents were for a long time fruitless. However, after sufficiently reliable methods of keeping records of new mutations were introduced, the American, Muller, and then other researchers, succeeded in obtaining them, first by the action of X-rays, and then with the help of other agents—ultra-violet rays, high temperature and chemical substances. By the use of these influences both chromosomal rearrangements of various orders and gene mutations were brought about. However, the result was always as 'indeterminate' as it is in nature. The results under experimental conditions were usually a repetition of the mutations which arose spontaneously in laboratory (or field) cultures. This enabled the geneticists to interpret the results of the experiments as an 'acceleration' of the natural process of mutation. However, in view of the complex structure and functions of the organism and the historical foundation of its reactions, we must agree with Darwin that the specific character of the change is always determined in a far greater degree by the individual peculiarities of the organism, its constitution, than by the character of the outside influence. It should not surprise us therefore that by applying *definite* factors we get *various mutations,* and that

by the action of *different* agents we in general get *the same mutations* as occur in nature.

"This does not mean that it is absolutely impossible to obtain determinate mutations. It is to be believed that in the end we shall succeed in obtaining specific hereditary variations by the action of definite factors at an exactly known stage of the development of the given organism (genotype), when it is in a definite physiological condition. That the physiological condition of the organism is also not a matter of indifference is shown by established cases of a considerable increase in mutations in seeds as they become older. . . ."

I go on to say: "There is no doubt that it is also possible to obtain other and more subtle determinate hereditary changes. This question is now ripe for study. Theoretically, we likewise entertain the possibility of parallel changes of the somatic and sex cells at stages when they are not yet specifically differentiated—viz., at the points of growth in plants." (*Problems of Darwinism,* pp. 220-21.)

From this it is perfectly clear that I consider that the source of variability lies in the external environment, but this variability is understood, of course, in the interrelation between the organism and the environment, and that the specific nature of the variation is determined more by the organism than by the environment, in view of the complex structure of the organism.

I am accused of stressing the indeterminateness of the variability of organisms. But I speak only of the indeterminateness of new variations, and not of the indeterminateness of reactions in general.

In the process of evolution, under the creative influence of natural selection, they are transformed into adaptive variations. Incidentally, the accusation was levelled at me that I distort Darwin by giving another definition of indeterminate variability.

I shall read my definition.

"Indeterminate variability, according to Darwin, means variations which are only indirectly connected with alterations in the external environment. He presumed the existence of reactions effected through the sexual system. But such reactions are not direct and, because of their complexity, cannot so

far be reproduced at will. Every individual reacts in its own way. The specific character of the reaction is chiefly determined by the individual properties of the given individual. These changes are, as a rule, inheritable. It is obvious that at bottom these are the changes which we now call *mutations*. However, there were also included among them combinations of changes and the non-adaptive modifications connected with them. In nature we practically always find that we are dealing with indeterminate individual differences."

"The Darwinian definition of the major forms of variation is the most happy of all existing definitions, since even the modern definition of modifications and mutations, as hereditary and non-hereditary changes, lacks sufficient clarity and has given rise to many misunderstandings." (P. 210.)

I have been charged with the view that evolution proceeds in a descending curve, in correspondence with the views of Daniel, Rose and other bourgeois theoreticians.

I shall read what I have written on this subject. The last chapter of my book, *Factors of Evolution*, deals specifically with the tempo of evolution. I shall quote a passage from the conclusion:

"Paleontology provides us with material which actually points to a rising tempo of evolution in the case of the more perfected and active organisms of any geological epoch. This applies, in particular, to the tempo of evolution of individual progressive phylogenetic branches. It is also true, however, of the whole process of evolution in general." (P. 382.)

It seems to me that I am the first of the Darwinists to note the acceleration, rather than the retardation, of the evolutionary process.

Incidentally, Prezent accused me of believing in reserve adaptation.

Never and nowhere have I said this, or could have said it, since I have constantly taken the geneticists to task precisely because I believe that all mutations are pernicious, and therefore could not have expressed a belief in adaptive mutations and their accumulation in a reserve.

I introduced the concept of a reserve of hereditary changes precisely in opposition to the idea of a gene fund. Whereas the gene fund is a static concept, the reserve is a dynamic

concept. In the reserve, not only is hereditary material used up, but there is a continuous accumulation of hereditary changes. In *Factors of Evolution* (pp. 191-92 and elsewhere), I speak in great detail of the accumulation of the reserve at the expense of mutation, spread of mutations, and their combination and conversion into complex hereditary variations. The accumulation of hereditary changes proceeds particularly freely under domestication; hence, in domesticated animals and cultivated plants we have the maximum accumulation of hereditary changes, a subject which I have analyzed in detail in my book, *The Organism as a Whole* (pp. 75-80).

I never have considered separate mutations valuable. I have written a special book to show that the organism evolves as a whole, and that only changes of the organization as a whole can be useful to the individual possessing them. Separate, partial changes cannot be useful. Hence, every mutation is harmful, and I could never have spoken of seeking out separate mutations, and still less have recommended it to breeders. I have always spoken of complex mutational changes. I shall give a few quotations so as to make this perfectly clear:

Problems of Darwinism, p. 223: "In this connection we should also specially note the fact that under natural conditions natural selection never deals with *individual* mutations."

On the following page: "If all mutations, taken separately, are as a rule harmful, i. e., are connected with the disturbance of established interrelations, this clearly shows that no mutation, *in itself*, is a stage on the road of evolution. The process of evolution must in no circumstances be conceived (as the adherents of the mutation theory do) as the result of a simple summation of mutations. Every mutation has first to undergo a certain transformation and combination under the guiding influence of natural selection." And, lastly, as the conclusion to this chapter: "Natural selection always has a sufficient field in which to manifest its action. Ordinarily, there can be no question of any inadequacy of material in the form of hereditary variations, i. e., mutations. However, we again stress, in the process of their accumulation they are refashioned. Combination in the process of crossing and the selection of the most favourable combinations in the progeny lead

to the result that every individual differs from another by very many individual peculiarities.

"Natural selection always deals with individual variations, i. e., differences developing on a diverse and always very complex basis. Not the least role in this diversity is played by non-hereditary changes, i. e., modifications." (P. 230.)

And now as to the last of the major accusations—the accusation of disarming practice. It will already have been seen from what I have said that this accusation is unfounded. I am charged with saying that "breed formation, rapid at the dawn of culture, gradually declines." You will not find that in any book, article or lecture of mine. I have never asserted that. The only passage, which was quoted here in part, says the following (*Factors of Evolution,* pp. 214-15):

". . . Both the formation of breeds of domestic animals and the formation of varieties of cultivated plants proceeded with such exceptional speed mainly, apparently, because of the previously accumulated reserve of variability. Further strictly directed selection is slower." At this point the quotation was broken off. But in the book it is followed by the statement: "although the possibility of eliciting more mutable lines, as well as of hybridization and the influence of external actions permits us here, too, to count upon far greater speed and not to set any restrictions in this respect (with the exception of the physiologically permissible limits)."

Hence, in the first place, there is no talk here of breed formation at the dawn of culture. It proceeds at all times. Recall the breeding of sugar beet for sugar content, which began only in the last century, and not at the dawn of culture. It very soon reached the limit. A limit, provided it is not a physiological limit, can be overcome, and I explicitly point to means of enhancing variability—hybridization and the influence of external factors, i. e., essentially the very same methods as were employed by Michurin.

The last accusation—why I do not speak of Michurin or of the achievements of other of our plant breeders. Very simply, because *Factors of Evolution* does not deal with these questions at all. If I thought of writing a book—and I may yet do so—on the question of governing variability and evolution, it would at all events be a bigger book than this. This is a special sub-

ject. Here I confine myself to an examination of the factors of evolution of animals and plants, and that only in order to substantiate the theory of "stabilizing selection." That is why in this special monograph I cite other works.

It is because of this that I practically make no reference to work done before 1920. Nor are there any references to the classics. I took the material which I needed to substantiate the theory of stabilizing selection, and nothing more.

Simultaneously with this book another book was published, *Problems of Darwinism*. Surely, I am not expected to repeat in one book what I have written in another. Here you will find references both to Timiryazev and to Michurin. Let us look into *Problems of Darwinism,* pp. 172-74 and pp. 238-40. Here I describe Michurin's major, and truly remarkable, achievements. I also devote sufficient space to the achievements of Academician Lysenko, Academician Tsitsin and other Soviet plant breeders. And my references to Timiryazev are even more plentiful. So I do not ignore our classics. It should be borne in mind that this is a special monograph, which bears the subtitle: "The Theory of Stabilizing Selection." And this is not a survey. I do not summarize even the contemporary material. This is material which is needed for the substantiation of the theory of stabilizing selection, and nothing more.

I think I have mentioned the chief reproaches levelled at me. Perhaps I ought to make one more remark.

Prezent reproaches me with attributing so much importance to hybridization. It should seem strange that Michurin also attributed great importance to hybridization. But, it appears, I make it an indispensable condition for the existence of evolution.

I am not so ignorant as not to know that in the bacteria, for instance, there is no sexual process, yet they evolutionize. I speak of the past importance of the sexual process and of crossing in the evolution of organisms, especially of the higher organisms. But this does not mean that it is an indispensable condition for all evolution. Hereditary variations appear independently of hybridization. Yet Prezent concludes that I attribute as much importance to hybridization as Weismann's theory of amphimixis does—that I am a Weismannist. Weis-

mann's theory of amphimixis assumed that the source of variability was crossing. I categorically deny this. I say that external factors are the source of variability, and I regard crossing as a means of accelerating the combination and synthesis of the manifestations of separate mutations. That is a different thing. It is obvious that my ideas have nothing in common with the theory of amphimixis. With this, permit me to conclude my elucidations of the remarks that have been made here about me.

Academician P. P. Lobanov. I call upon I. N. Simonov, Master of Agricultural Sciences.

I. N. Simonov (Ministry of Higher Education). I am speaking here not as a representative of the Ministry of Higher Education, but as a scientific worker, Master of Agricultural Sciences and Docent.

Since Academician V. S. Nemchinov, who is present here, has not spoken yet, I would like to tell this session of the Academy something about the work of our Timiryazev Academy. As you know, 1950 will mark the 85th anniversary of the foundation of the Timiryazev Agricultural Academy. The history of this Academy is, in fact, the history of a big division of the agronomic science of our country. Here for many years coryphaei of the science of biology like Timiryazev and Williams carried on their work. As is well known, the Timiryazev Academy was the pioneer in the field of plant breeding. Here Academician Williams was the first to lay the foundations for the breeding of agricultural plants.

Justice compels me to say, however, that in matters of plant breeding, and of agronomic science in general, the Timiryazev Academy today lags far behind many scientific research institutes, and even behind some college scientific bodies.

Is the Timiryazev Academy today, with its large staff of scientists and all its splendid opportunities, what it really should be, namely, the magnetic centre of the Timiryazev and Michurin trend in science? In answer to this question I take the liberty to say that the Academy is not such a centre today. To prove that this is not merely a bald statement I will quote several examples.

In 1937, that is, about ten years ago, some of the research

workers of this splendid Academy—postgraduate students and students—plucked up courage and travelled to Odessa to visit the Institute of Selection and Genetics, of which T. D. Lysenko was then Director. When they returned they wrote an article for the Academy newspaper *Timiryazevka* entitled "For a Radical Reorganization of Scientific Research Work."

One would have thought that this would have marked a turning point in the work of the Academy. But there was no turn, there was no reorganization, and the work went on in the old way right up to these very last days. There was a "reorganization," but in an altogether different direction. All those who shared the views of Michurin and Academician Lysenko were, on various pretexts, "promoted," transferred to other institutions, or simply dismissed. Many scientific workers were treated in this way: E. I. Ushakova, now an Academician, Professor Veprikov, the Michurinist fruit grower Pavlova, Tikhonenko, the student Alisov, and many others.

Things reached such a pass that Academician V. S. Nemchinov—he knows this very well—simply expelled from the Academy, teachers he considered undesirable who had been appointed to the chair not by chance, but by deliberate choice from a number of applicants and had been recommended by I. V. Michurin; he expelled them only because Michurin or his pupils had some time, somewhere, spoken well of them.

There was a time when of the large staff of professors and teachers at the Timiryazev Academy not a single one dared to accept the pressing invitation of the students to attend the celebration of Michurin's jubilee, the jubilee of our great Russian scientist. Students like Gritsenko, Barkov, Golub and Simonov gathered together and travelled alone to Michurinsk to do honour to the great Russian scientist and innovator.

There were also times in the life of the Timiryazev Academy when students and postgraduate students who expressed a desire to study directly under Michurin, could do so only at their own expense. They went to Michurinsk to learn from Michurin himself the methods he employed.

Michurin's letter to a students' circle at the Moscow, now Timiryazev, Agricultural Academy, published in the first volume of his works (p. 244) in which he emphasized that ... "complete unfamiliarity with the subject breeds a too neglect-

ful and sometimes a downright ignorant attitude towards it," remained forgotten.

This was the situation eight years ago. Do you think any change has taken place at the Timiryazev Academy since then?

Academician I. V. Yakushkin, whom we all esteem, quite rightly said in his speech here that the number of adherents of the non-Michurinian trend is steadily diminishing. This, of course, may be said of the body of scientific workers in the Soviet Union as a whole, but it would not be quite correct to say this of the body of scientific workers at the Timiryazev Academy. Nor can it be said that the Director of the Academy is a Michurinist. Academician Yakushkin, Professor Timofeyev, Academician Bushinsky, Professor Chizhevsky, Professor Edelstein and several of their colleagues may be said to belong to the Michurin trend. But taken as a whole, as regards the whole body (I have in mind the staff of professors and teachers), it would be somewhat wrong to say this.

Why is it not possible to say this about all the scientists at the Timiryazev Academy? If it were possible, the first one to say so from this rostrum should have been not a rank-and-filer, but the Director of the Academy, Academician V. S. Nemchinov. But why has Academician Nemchinov been unable to come up to this rostrum and tell us what changes have taken place in the views of the scientists at the Timiryazev Agricultural Academy during the past few years? And who, if not Academician Nemchinov, should have set resolutely to work to reorganize the study of the Michurin heritage at the Timiryazev Agricultural Academy. I boldly assert that the Director of the Academy himself shares the views of Morganist genetics. From where, from which colleges, and which chairs were articles roughly attacking Academician Lysenko, sent to the hidebound reactionary press in America? Not from just anywhere, but from the Timiryazev Agricultural Academy. Such articles, as is known, were written by Professor A. R. Zhebrak, who spoke here only yesterday. This was encouraged by the policy pursued by Academician Nemchinov of supporting such scientists. Scientists like Zhebrak, and others, apparently, have not to this day (*commotion*), I have a perfect right to say this

judging by the speeches they delivered yesterday, freed themselves from subservience to foreign science.

The attacks Professor Zhebrak levelled against Academician Lysenko in the bourgeois press are, in fact, attacks upon the teachings of Michurin. He himself knows that perfectly well. These attacks are attacks upon the teachings of Timiryazev and Williams. But did any change take place after Soviet scientific public opinion censured Professor Zhebrak's behaviour? No. I can boldly say that judging by the speech Professor Zhebrak delivered here yesterday, little has changed at the Academy.

Let us, comrades, recall the celebrated session of horticulturists that was held two years ago. Many of you present here today witnessed the obstruction that was raised by some of the students of the Timiryazev Academy against Academician P. N. Yakovlev, Michurin's best pupil. You, esteemed Academician Nemchinov, as Director of the Academy, undoubtedly bear moral responsibility for the training of your students in a totally wrong direction. . . .

A voice. Not only moral, but political.

I. N. Simonov. Yes, and political responsibility.

I will quote another example to show in which direction—the Michurin or some other—the Timiryazev Academy is going today. This example is really worth quoting.

Two years ago applications were called for by our Timiryazev Academy to fill a chair that one would think was remote from the present controversy—the chair of the technology of fruit and vegetable plants. One of the applicants was B. A. Rubin, one of Bach's pupils, a biochemist, a professor, and now holder of a Stalin Prize. But he was not elected. His application was rejected not because he lacked the scientific qualifications for a post at the Timiryazev Academy, but because he subscribes to Michurin's teachings. They did not want to "pollute" the precincts of the Timiryazev Academy with persons of that kind. I could tell a long story about the state to which you, Academician Nemchinov, reduced M. A. Pavlova, the experimental horticulturist.

Perhaps changes have taken place in scientific research work at the Timiryazev Academy, in the study of the heritage left us by Michurin, Timiryazev and Williams, at least during

the past few years? There is no evidence of this, and the reason is that the Director of the Academy himself is a supporter of the reactionary Morganist theory.

I will say more. Last year, with the knowledge of Academician Nemchinov, the last base for practical plant breeding by students was liquidated. . . .

Voices. Hear, hear!

I. N. Simonov. This was a collection of European and Russian gooseberries and currants. Material which had just reached the real fruit-bearing stage (ten-year-olds) was sold by auction. Those who witnessed the scene could not leave that spot without emotion. The collection was sold by auction! Perhaps you will say, Academician Nemchinov, that the orchard with fruit trees survived. Yes, that orchard survived by a strange accident, although the fence was already knocked down by a tractor. But permit me to ask Academician Nemchinov, what work is being done in that orchard? None! Ordinary tree planting, ordinary agrotechnical operations, and nothing more. No creative plant-breeding work is being conducted. Evidently, this fully suits the book of Academician Nemchinov and some of those of his like-minded friends who managed to disperse a fine body of scientific workers. The orchard is flourishing—and that's all! But is that what you call studying the Michurin heritage?

I can quote a passage from Michurin's works which shows what he meant by studying his legacy, and what he meant by ordinary tree planting. In volume IV of his *Works*, p. 192, Michurin writes:

"Any horticulturist can cultivate ready-made varieties, but new varieties can be created only by one who knows the ways of nature's evolutionary work which produce a constant succession of varieties of living organisms and never permit a repetition of old forms."

That is how Michurin calls upon us to study his legacy. We must not simply shift trees from one place to another, but assimilate and further develop Michurin's methods. But Academician Nemchinov must be unaware of these pages, as many of his like-minded friends are unaware of them.

I can quote still another example. Five years ago, a small group of workers at the Michurin Institute who were working

under my direction drew up a syllabus for plant breeding and genetics, as the old syllabus for agricultural colleges needed thorough revision in the light of the teachings of Michurin and Timiryazev. The group spent three or four months on drawing up the syllabus. When it was ready it was sent to Academician Nemchinov at the Timiryazev Academy for his opinion. The group received no reply. And only in the 1948/49 term will students at agricultural colleges begin to study plant breeding and genetics according to the new syllabus.

That is why I think that the Timiryazev Academy, directed by Academician V. S. Nemchinov, must radically reorganize its work so as to justify its name as the Timiryazev Academy.

The works of Michurin, Timiryazev and Williams must be closely studied not only at agricultural colleges, scientific institutes and experimental stations, but in all the colleges and universities throughout the country.

Judging by what Professor A. R. Zhebrak said in his speech, however, it is evident that he has no intention to speak about the progressive theories of Timiryazev and Michurin, about Soviet biology in the Genetics Department.

Some of the speakers have accused Academician Lysenko of fighting only for vegetative hybridization, of applying Michurin's legacy one-sidedly. This is sheer calumny. It is sufficient to read Academician Lysenko's ten-page preface to the first volume of Michurin's works to see what tasks he indicated in assimilating Michurin's theory.

Academician Lysenko has always called for a deep and comprehensive study of the works of Michurin and Timiryazev, including the applied branches—the chemistry, biochemistry and physiology of plants. I still have my notes on the lectures I heard Academician Lysenko deliver when he was still in Odessa. I very well remember, and my notes confirm it, that Academician Lysenko urged us to study the cell, to study its diversity of quality, and to convince ourselves of this by peering into its interior with the aid of the microscope.

This is what Academician Lysenko urged his students to do. And in many other of his lectures and talks, and also in the press, he always calls for a comprehensive study of the works of Michurin. Academician Lysenko stands for the new, progressive teachings.

It is entirely unbecoming for any of our scientists to bolster up the fictitious laws of Mendel and Morgan and to forget about the rich heritage of Michurin, Timiryazev and Williams.

The names of the distinguished Russian scientists Williams, Timiryazev and Michurin will live forever in the hearts of our multinational Soviet people and of the whole of progressive mankind. (*Applause.*)

Academician P. P. Lobanov. I call upon Comrade Malinovsky, Master of Biological Sciences, of the Institute of Cytology of the Academy of Sciences of the U.S.S.R. Is Comrade Malinovsky not here? In that case I call upon Academician S. F. Demidov.

Academician S. F. Demidov. The present session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. will play an extremely important role in the further development of agronomic science in our country. The questions under discussion are matters of considerable importance in principle for other branches of science besides selection and genetics, the theory of plant and animal breeding.

No, the problems cover a much wider field.

In his address, Academician Lysenko called upon us to conquer new heights. He gave us the theoretical basis for and indicated the ways of further developing agronomic science as a whole. His address provides the scientific basis for the entire work of our scientific institutions, of all the agricultural and planning bodies, and of state farms and collective farms in their efforts successfully to carry out the main tasks set by the Five-Year Plan Law and the historic decisions of the February Plenum of the Central Committee of the C.P.S.U.(B.) to increase the yields and gross harvest of our agricultural crops, and to increase the number of our livestock and raise their productivity. In my opinion, it is impossible to agree with those scientists who in their speeches here tried to reconcile the two trends that have definitely revealed themselves in the science of biology, trends which spring from opposite ideological sources.

Thus, B. M. Zavadovsky, who spoke here, lamented the harsh and blunt terms in which the discussion of the situation

in the science of biology is being conducted at this session. He would not agree that this is a conflict between two diametrically opposed trends in the science of biology and claimed that there is a third, a middle trend. Academician Zhukovsky also insisted that both trends in biology be allowed to develop, and he called upon us to give Mendel and Morgan their due. He said here that he saw nothing prejudicial in the development of several different schools in science.

I would like to ask Academician Zhukovsky which schools he is referring to, what has he in mind? We must clear up this question. We stand for the development of the progressive trend. The Soviet people are well familiar with scientific schools like that of V. R. Williams in soil science and agriculture, the school of Academician D. N. Pryanishnikov in agrochemistry, the school of M. F. Ivanov in zootechnics, and the school of Academician V. P. Goryachkin in the employment of machinery in agriculture. But everybody is aware that it is not these that are referred to in the discussion on the two trends in the science of biology.

In his address, Academician Lysenko made this point perfectly clear, namely, that the two trends that have arisen in the science of biology are fundamentally distinct, diametrically opposed to each other. One trend is the genuinely scientific, progressive, Michurin trend; the other, on the contrary, is the unscientific, reactionary-idealistic, Weismannist (Mendelist-Morganist) trend.

The first-mentioned trend, dialectical-materialist, creative, marches in step with the requirements of life, is in complete harmony with our Marxist-Leninist world outlook, is developing on the basis of the all-conquering theory of Marx, Engels, Lenin and Stalin. The second trend, on the contrary, drags us towards mysticism, it is the straight road to clericalism, it tries to direct practical socialist agriculture into false channels and causes confusion in the ideological training of our cadres.

This is how the question stands, Academician Zavadovsky and Academician Zhukovsky! And all attempts to reconcile these conflicting trends in the science of biology, all attempts to occupy some sort of a middle position between the scientific Michurin trend and the unscientific Weismannist (Mendelist-Morganist) trend are inevitably doomed to failure, they

will be defeated. Lenin and Stalin teach us that a conflict between two trends must be settled not by conciliation, or by seeking some kind of a third, middle trend, but in a sharp, open battle of principles.

Precisely such a solution of the problem is dictated primarily by the interests of our socialist agriculture, in order to secure the further growth and prosperity of our collective farms and state farms.

Indeed, take the task that the present stage of development of production in our collective farms and state farms sets us in all its magnitude, namely, of introducing on a nation-wide scale the travopolye system of agriculture, the system combining the principles of Dokuchayev, Kostychev, and Williams. The best minds in Russian agronomic science, scientist agronomists like Sovyetov, Dokuchayev, Kostychev, Timiryazev and Williams dreamed all their lives of obtaining big, stable crops. In the course of the age-long struggle waged by Russian agronomics a splendid and harmonious theory was worked out of restoring and increasing the fertility of the soil, of eliminating the catastrophic drop in crop yields in years of drought. Those coryphaei of Russian science worked out the theory of the travopolye system of agriculture that was generalized and splendidly expounded as an integral science by Academician Williams. The successful application of this system in the Volga districts, in the North Caucasus, in the Ukraine, in the Central Black-Earth Belt and other steppe zones of agriculture in our country is producing exceptional, positively amazing results.

Everybody is aware of the brilliant results achieved in the introduction of the travopolye system of agriculture on large areas by the Dokuchayev Institute of Agriculture of the Central Black-Earth Belt, in the Salsk and Millerovo districts in the Rostov Region, in the Novo-Annensk District in the Stalingrad Region, in the Buzuluk District in the Chkalov Region, and in the Gigant, Kuban and other state farms.

Our agronomic science and practical, socialist agriculture are now faced with the urgent task of applying the combined Dokuchayev-Kostychev-Williams system all over the country.

In order to carry out this immense task successfully, we must concentrate all our efforts and material and technical

resources on the propaganda and practical organization of the lea crop rotation—both field and fodder crops—of the system of operations necessary for the proper cultivation of the soil, the planting of shelter belts, and the digging of ponds and irrigation canals to tap local water resources, and other measures.

And now picture to yourselves the other schools which will come along and argue the opposite, which will throw doubt on the fundamental principles of the travopolye system of agriculture. What will come of this? The retardation and disruption of these extremely important measures of state significance. Unfortunately it must be said that the opponents of the travopolye system of agriculture, the representatives of the other "scientific" school, like Academician Zhukovsky, say, have done quite a lot in our colleges and universities, scientific research institutes and agricultural administrations to hinder its triumphant march.

The same applies to the Michurin trend in biology. I. V. Michurin made immense discoveries which caused a veritable revolution in science, and the further progress we make towards Communism, the further progress we make towards an abundance of agricultural produce, the more will the beauty and magnitude of the theory evolved by Michurin's genius be revealed.

Based as it is on the principles of dialectical materialism, the Michurin trend in biology is constantly developing Darwinism and, as T. D. Lysenko said in his address, is casting aside the obsolete canons and fallacious propositions that Darwin propounded. The Michurin trend in biology is creating new varieties of cultivated plants and new breeds of farm animals, it is further developing the fundamental problems of biology in inseverable connection with practical socialist farming. Michurin's long and fruitful life is a model of heroic achievement, of devoted service to the people, a magnificent illustration of the unity between theory and practice.

The central problem, the solution of which will arm our collective farms and state farms, our plant breeders and agronomists with powerful weapons that will enable them to display all their constructive daring in the struggle for high yields and high productivity of livestock, may be formulated as follows: the Michurin trend in biology proceeds from the

proposition that changes in the conditions of existence, in environmental conditions, cause the new conditions, sooner or later, but without fail, to break the old type of development of plants and animals and to create new varieties corresponding to the new conditions of existence.

The Michurin trend in biology proceeds from the premise that the new characters that plants and animals acquire as a result of a change in the conditions of existence can be transmitted by heredity.

Herein lies the great progressive significance of Michurin's work, for the proof by the Michurin trend of the inheritance of acquired characters by plants and animals, equips practical workers with scientifically tested methods of altering the nature of plants and animals and of producing new varieties of agricultural crops and new breeds of animals.

It is this that gives us every right to say that Michurin's theory is, in principle, a new stage in the development of the theory of evolution and the science of biology.

In its very spirit Michurin's theory is creative, creatively active. Michurin surmounted Darwin's error in paying tribute to Malthus' false theory of the overpopulation of organisms in nature; he surmounted the passive, contemplative character of Darwin's theory of evolution. Michurin laid the foundation for the transformation of the plant world, for the creation by man of new varieties of cultivated plants. This created the conditions for the realization of Darwin's dream of man creating new and more productive varieties of plants and animals.

In his *Origin of Species* Darwin wrote: "A new variety raised by man will be a more important and interesting subject for study than one more species added to the infinitude of already recorded species."[1]

The great transformatory power of Michurin's theory lies in that it calls for active intervention in nature's activities. Michurin opened up unprecedented vistas and pointed to the alluring prospects of accelerating the formation of new varieties and strains of plants and new breeds of animals and of turning their development in the direction most useful to man. "The most urgent task that confronts us today," said Michu-

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[1] Ch. Darwin, *The Origin of Species*, London 1861, p. 521.

rin, "is to find the means by which we can more easily and more successfully intervene in nature's activities and thereby reveal her 'secrets'."[1]

The Weismannist trend in biology denies that external conditions influence the formation of an organism's inherited characters, and it also denies that the new characters and qualities which organisms acquire as a result of a change in their conditions of existence can be transmitted to their offspring.

The problems we are discussing are extremely important in revealing the reactionary, metaphysical essence of Weismannism (Mendelism-Morganism), for to deny that the conditions of existence influence the specific nature of the inherited variations of an organism means harmfully separating plant breeding from agrotechnics in plant cultivation, it means separating the work of improving the breeds of farm animals from measures to ensure fodder supplies and to improve the conditions under which animals are kept.

This shows that the conflict between the two trends is a matter of burning importance for practical workers in our socialist agriculture.

The characteristic feature of genuine progressive agronomic science is that it equips practical workers for the task of raising socialist agriculture to a still higher level, of increasing the yields of agricultural crops and of the productivity of livestock.

Such a science is the Soviet agrobiological science created by Michurin and Williams and successfully developed by Academician Lysenko.

One of the most important services Academician Lysenko has rendered is that, in solving the important practical problems connected with the development of socialist agriculture, he has skilfully combined the theories of Timiryazev and Michurin on the creation of varieties and the alteration of the nature of plants and animals with the theories of Dokuchayev, Kostychev and Williams on soil formation and methods of increasing the fertility of the soil, thereby blazing new trails in agronomic science.

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[1] И. В. Мичурин, *Сочинения*, том I, стр. 410.

Soviet agrobiological science, created by Williams, Michurin and Lysenko, has proved to be extremely fruitful. This science has worked out radical measures for increasing yields such as:

a) correct lea crop rotation of field and fodder crops;

b) a scientifically tested system of main and pre-sowing tillage—winter and summer fallow;

c) a correct system of organic and mineral fertilizers, determining the place of fertilizers in the crop rotation;

d) a system of measures for improving seed and for producing new varieties of agricultural crops and new breeds of animals;

e) planting steppe shelter belts, combatting ravine formation and other forms of soil erosion;

f) development of irrigation and the proper organization of irrigation works by utilizing local water resources;

g) employment of machines in agriculture suitable to the specific features of each zone—machines commensurate with advanced agrotechnical science.

All these measures are being introduced in our collective farms and state farms on an ever increasing scale and enable us year after year to increase our crop yields and the productivity of our cattle.

Academician Lysenko himself, and those working under his direction, have performed important work of the utmost significance in helping to increase yields; and this work is widely known among practical workers on our collective farms and state farms. In the state plans for the development of agriculture which our government adopts every year, the methods proposed by Academician Lysenko take up a whole section. The same can be said with respect to the long-range plans. These plans are drawn up for regions and republics and are sent to the collective farms and state farms. Thus, millions of collective farmers and workers on state farms are familiar with the work of Academician Lysenko and his assistants. The scale on which the methods proposed by Academician Lysenko are being employed in production is very considerable. I will mention a few of them:

1. Vernalization of cereal crops which makes it possible to extend the cultivation of valuable varieties of spring wheat to

more northerly districts and ensures a considerable addition to the crop. This method is now being widely employed on our collective farms and state farms. In 1940 vernalized seed was sown on an area of 13,000,000 hectares. In 1948, in conformity with the decision of the February (1947) Plenum of the Central Committee of the C.P.S.U.(B.), vernalized seed is to be sown on an area of 7,000,000 hectares. Vernalized potatoes are also planted on a very considerable area.

2. Summer planting of potatoes, which will put a stop to the degeneration of this crop in the southern regions. This method is already being employed on an area running into hundreds of thousands of hectares. The summer planting of potatoes increases the yield and improves the quality of this crop which is so valuable for our national economy.

3. A number of high-yield varieties of agricultural crops have been produced. The yield of the winter wheat Odessa 3 produced by the Odessa Institute of Selection and Genetics, under Academician Lysenko's direction, exceeds that of the standard varieties by 3-4 c. per ha. This variety is both frost and drought resisting. The spring barley Odessa 9 is also grown on a large area. The cotton variety Odessa 1 is practically the staple crop for the new cotton growing regions. Academician Lysenko played an important part in working out the scientific principles of seed cultivation in our country.

4. Measures for fortifying our own raw materials base for the production of natural rubber, the cluster sowing of kok-saghyz, the vegetative propagation of kok-saghyz, the cultivation of tau-saghyz in the southern regions of the U.S.S.R. and the elaboration of a series of agrotechnical measures for the cultivation of kok-saghyz.

5. Special mention must be made of the extensive employment of methods for increasing the yield of millet. As far back as 1940, the Lenin Academy of Agricultural Sciences, under the direction of Academician Lysenko, proposed a carefully thought out schedule for the sowing, tending and harvesting of this crop and the methods by which this was to be done. This plan was put into operation in 1940 on an area of over 500,000 ha., and in 1947 on an area of over 1,000,000 ha., and a yield was obtained of over 15 c. per ha.

6. The topping of cotton plants is now practised on 85 to

90% of the total cotton area. This prevents the dropping of buds and increases the pre-frost harvest of the best grades of cotton by 10 to 20%.

7. During the Great Patriotic War Academician Lysenko proposed methods for increasing the sprouting of cereal-crop seeds in the eastern regions of the U.S.S.R. The adoption of these methods helped the collective farms and state farms in Siberia greatly to increase their own seed supplies and to raise the yield. The methods worked out by Academician Lysenko for introducing the cultivation of winter wheat in the Siberian steppe regions have now been tested by practice and have been approved by agronomists and by the local Party and Soviet administrative organizations, in particular by the Altai Territory Committee of the C.P.S.U.(B.) and by the Executive Committee of the Altai Territory Soviet. (*Applause.*)

8. Representatives of the Michurin trend in biology have worked out and extensively employed in practice such an effective method of plant breeding as intravarietal and intervarietal crossing, culling in the process of selection and the deliberate choice of parental pairs.

9. In conformity with the decisions of the February Plenum of the Central Committee of the C.P.S.U.(B.) the summer sowing of alfalfa in bare fallow is now being widely practised in the steppe regions in the South. This ensures a large increase in the seed crop of this grass, which is so necessary for the introduction of a correct lea crop rotation.

10. During the war, Academician Lysenko drew up an improved schedule for the sowing and harvesting of crops in Siberia which is now extensively adhered to by collective farms and state farms. He also proposed important measures for combatting beet weevil, the utilization of the upper parts of potato tubers for planting, which greatly increased the planting resources for this crop, the biological method of combatting pests, and a number of other measures. I deem it necessary especially to note that Academician Lysenko is successfully solving the problem of introducing the cultivation in the U.S.S.R. of branched wheat, and also the problem of the afforestation of the steppe regions. The first steps taken in this direction are very promising. All the measures I have enumerated, and many more, show that one of the fundamen-

tal features of Academician Lysenko's scientific work is that he is in daily contact with the collective farms and state farms, that he enlists the cooperation of a large body of the foremost workers in agriculture in his scientific researches, and secures the speedy introduction of new scientific discoveries in practical agriculture. In this he sets an example to every Soviet scientist and to every patriot. The experience of thousands of foremost collective farms and state farms and of entire districts shows convincingly that the application of the measures elaborated by Williams, Michurin and Lysenko is one of the most important conditions of success in the work of our collective farms and state farms. That is why our annual and long-range state plans for the development of socialist agriculture unfailingly include the instruction to introduce the measures proposed by Williams, Michurin and Lysenko, i.e., the achievements of progressive agronomic science.

Every year the Government adopts plans for ploughing and cultivating black and bare fallow, for ploughing with plough and skim coulter, for mulching immediately after reaping, plans for sowing vernalized seed for cereal crops, for the summer planting of potatoes in the southern steppe regions, for the introduction of correct lea field and fodder crop rotations, for the planting of shelter belts, for developing irrigation works on the basis of local water resources, and for extending the cultivation of the high-yield varieties of agricultural crops that have been produced by the representatives of the Michurin trend in the science of biology.

It is also necessary to say very definitely at this session of the Academy that the opponents of the progressive, genuinely scientific Michurin trend in biology, the representatives of the reactionary idealist trend, the Weismannists (Mendelist-Morganists) have produced nothing of value for the development of socialist agriculture.

The representatives of formal genetics—Professor A. R. Zhebrak, Professor M. S. Navashin, Professor N. P. Dubinin, Academician B. M. Zavadovsky, Docent S. I. Alikhanian, Docent S. D. Yudintsev, and others—confine themselves to fruitless laboratory experiments on fruit flies and to cultivating tetraploids and polyploids. What could the Weismannists

(Mendelist-Morganists) demonstrate to us, what could they tell us about from the rostrum of this session? Absolutely nothing! Except, perhaps, about the tetraploid grains of buckwheat in test tubes, and about the flies in Voronezh which Professor N. P. Dubinin studied and found that their chromosome apparatus had changed as a result of the war. (*Laughter.*)

I regret to say that some of our scientific institutions have not yet turned their attention to the practical needs of our socialist agriculture; they are still engrossed in "researches" of extremely doubtful scientific and practical value.

This applies in particular to a number of institutes and laboratories of the Biological Department of the Academy of Sciences of the U.S.S.R., the highest scientific institution in the country.

The divorcement from life, the sterility and reactionary nature of the Weismannists (Mendelist-Morganists) can be vividly illustrated by the contents of the latest book by Academician I. I. Schmalhausen, *Factors of Evolution,* published by the Academy of Sciences of the U.S.S.R. in 1946.

In this book Academician Schmalhausen regards the formation of breeds of farm animals and of varieties of cultivated plants as the search for the hidden reserves of mutations that were accumulated in the wild state in the initial stage of the cultivation of the given plant or animal. These reserves of mutations or "reserves of variability" as the author calls them were, according to Schmalhausen, only discovered in the historical process of formation of the breed or variety and not created by cultivation. Proceeding from this, the author asserts that the process of formation of breeds and varieties was most rapid at the dawn of culture and that subsequently "...directed selection is slower...." (P. 215.) This thesis, one of the most important in Academician Schmalhausen's book, runs counter to the facts and disarms practical plant and animal breeders in their work of producing new varieties of agricultural crops and new breeds of animals, for it suggests that the "golden age" of plant and animal breeding has passed away.

Academician Schmalhausen is unable to explain from the point of view of this theory the causes of the formation of numerous breeds of pigeons and the existence of a small number of domesticated birds—geese and ducks—that he de-

scribes in his book. According to Darwin, the limited number of breeds of geese and ducks is due to the one-sided interests that prompted man when breeding these birds (geese and ducks were bred only for their meat, eggs and feathers) and the numerousness of the breeds of pigeons is due to the diversity of interests that prompted man in breeding them (various economic needs, sport, war, postal, decorative and other purposes). According to Schmalhausen, however, the smallness of the number of breeds of geese and ducks is due to the fact that they possess "a smaller reserve of variability." The logical deduction from this is that pigeons represent the most progressive branch of the animal kingdom, while geese and ducks represent the non-progressive branch, since they lack an adequate "reserve of variability." Such are the reactionary and absurd conclusions that one can be driven to by extremely fallacious fundamental theoretical principles in biological science.

Previous speakers have already shown what political blindness one can be afflicted with if one adheres to false theoretical premises. In his book *Factors of Evolution,* which Professor Polyakov and Professor Paramonov described in their reviews as marking "a new stage in the development of Darwinism," Academician Schmalhausen loudly advertises the reactionary idealist theory of Mendel-Morgan, praises and recommends the works of reactionaries like Dobzhansky and Timofeyev-Ressovsky, gives a long list of other little-known authors, but does not say a word about Michurin, and completely ignores Timiryazev; the works of Michurin and Timiryazev are absent from the list of recommended literature. In a book which, as Schmalhausen claims, is a synthetical study of the materials of present-day genetics and ecology, no mention is made of the works of Academician B. A. Keller, one of the founders of present-day plant ecology.

Michurin, the pride of Russian, Soviet biology, and the world-famous works of Academician Lysenko, are utterly ignored by Schmalhausen in his book *Factors of Evolution.* All this is unworthy of a Soviet scientist. I cannot help mentioning in this connection the unseemly behaviour of Professor Polyakov who, after having written a laudatory review of Academician Schmalhausen's book, declares at this session that it is the

least successful of the books that Schmalhausen has written. He and Professor Paramonov said in their reviews that Academician Schmalhausen's book *Factors of Evolution* marks a new stage in the development of Darwinism, but when investigated, this new stage turned out to be an attempt to resuscitate the old Weismannist ideas. The Moscow University and the Biological Department of the Academy of Sciences expressed a high opinion of Academician Schmalhausen's *Factors of Evolution*. The fact that this book meets with the full approval of the Biological Department of the Academy of Sciences and the Scientific Council of the Moscow University shows that all is not well in some of the institutes of the Academy of Sciences of the U.S.S.R. and also in the Moscow University. I cannot but express surprise at the behaviour at this session of Academician V. S. Nemchinov, who utters loud interjections of approval when the Weismannists (Mendelist-Morganists) speak, and of indignation when the latter are criticized by representatives of the Michurin trend, and by Academician Lysenko in particular. It is well known that not all is well at the Timiryazev Academy of Agriculture as regards the study and propagation of Michurin's teachings. For a number of years the Genetics Department of this, the largest agricultural college in the country, has been propagating Mendelist-Morganist ideas. I hope Academician Nemchinov will speak at this session and tell us clearly which trend in biology the Timiryazev Academy will adhere to in future.

Voices. Hear, hear! (*Applause, commotion.*)

S. F. Demidov. The extensive practical work that is going on in our socialist agriculture, the vast army of outstanding men and women who have achieved high yields of agricultural crops and high productivity of livestock—our Heroes of Socialist Labour—and the achievements of our scientists prove up to the hilt that the Michurin trend in biology is the most progressive trend in this science, the trend that arms our practical workers for the struggle to achieve high yields, to raise our livestock industry to a higher level and to create an abundance of agricultural produce. Basing himself on the principle of materialist dialectics that the living came from lifeless nature, Michurin always pointed in his works to the determining influence which the conditions of existence, envi-

ronmental conditions, exercise upon the development of plant and animal organisms. Rightly proceeding from the premise that changes in the heredity of the living body are due to the action of the conditions of its existence, Michurin proved, not only in theory but also by extensive practical experiments, that by utilizing every means of actively influencing nature, man can create new varieties of plant and animal organisms, can lend them hereditary qualities. The Michurin trend in biology has discovered new ways of acclimatizing plants that are needed in our national economy, and has defined the methods by which plants can be shifted to new regions.

The measures being taken in our country to develop seed cultivation, to increase the fertility of our collective-farm and state-farm fields, to plant shelter belts, to ensure high and stable yields in drought years, and other measures, cannot be successfully carried out without the further, all round development of the theories of Williams, Michurin and Lysenko.

In the light of Academician Lysenko's address that we heard here, and of the debate on the address at this session, the proper direction of scientific research and the methods of teaching the biological sciences at our colleges and universities acquire exceptionally urgent importance. These matters have direct bearing on the moulding of the scientific outlook of our cadres. In our scientific research institutes, laboratories and experimental stations, and in the teaching of biology, the Michurin trend must have undivided sway. The combined systems of Dokuchayev, Kostychev and Williams, the theory of the travopolye system of agriculture adapted to the specific features of each agricultural zone—which is the pride of Russian, Soviet agronomics—must be brought to the knowledge of our agronomists and organizers and be taught daily in our colleges and universities.

We must as quickly as possible put a stop to the situation in which agricultural colleges, including the Timiryazev Academy, often turn out agronomists who have thoroughly learned Mendel's "pea laws," as Michurin called them, but who have a very vague idea about the creative methods of altering the nature of plants and of producing new varieties that were worked out by Michurin and are being developed by Lysenko.

Permit me, in conclusion, to express the conviction that the Academy of Agricultural Sciences of the U.S.S.R. which bears the great name of Lenin, reinforced by new members, will become a powerful centre for the development of progressive agronomic science in our country and that it will always have inscribed on its banner the words of Comrade Stalin: "Science is called science just because it does not recognize fetishes, just because it does not fear to raise its hand against the obsolete and antiquated, and because it lends an attentive ear to the voice of experience, of practice." *(Prolonged applause.)*

Academician P. P. Lobanov. I call upon Professor D. A. Kislovsky.

Professor D. A. Kislovsky (Timiryazev Academy of Agriculture). More than ten years ago, in 1936, if I am not mistaken, at the IV session of the Lenin Academy of Agricultural Sciences, I subjected formal genetics to comprehensive criticism and pointed to the harm it was doing to practical farming. At that time we entered into mortal combat with what seemed a virile and powerful foe. One of our opponents was Professor Muller, a pillar of American genetics, who came out not as a speaker in the debate, but with a long address. Many of us who heard him then thought that we had plunged into an almost hopeless fight. Nevertheless, we won the day, and formal genetics was knocked out of the saddle. But formal genetics still holds many of its positions. It has entrenched itself and is waiting for better days.

During the past ten years our Soviet people and our Soviet system have displayed strength and firmness that have astonished the entire capitalist world; the warmongers never suspected that we were capable of this. Our Soviet State not only withstood the furious onslaught of Hitlerism, but passed to the offensive and vanquished the fascist beast. The imperialists realized that Socialism is not only a slogan, but a force with which they have to reckon.

Much has also changed in the field of biology during these ten years. We no longer see strong and confident opponents. The enemy now prefers manoeuvring to frontal attacks. But the struggle on the theoretical, the scientific front is still in

progress, and we must continue to wage it with unabating ardour and devotion to principle. We must bear in mind that the fight is not over minor issues, but over the fundamental problems of Soviet science, of Soviet ideology. The remnants of reactionary ideology that have still remained in the field of biology must be finally liquidated.

One of the speakers here, it was the representative of the State Planning Commission, if I am not mistaken, said: "A refreshing thunderstorm burst over science." It is wrong to think, as some do, that that thunderstorm burst only over formal genetics. It burst over all those who, confined to their musty studies, are lagging behind the development of Soviet, socialist life and want to go on dabbling in Soviet science in accordance with the prescriptions of reactionary science. (*Commotion.*)

The issue in this discussion is the fundamental methodological line to be followed in Soviet science in general, and not only in some department of biology. What kind of logic is bourgeois natural science based on, and ought we make an exception in the case of bourgeois natural science compared with the rest of the heritage of capitalism, the technical heritage, say. We have grown accustomed to reworking, to assimilating the heritage of the past for the benefit of our social system, and to developing it in the process of assimilation. We need natural science not for the museum, but for life. We want to develop not only the productivity of our plants and animals but also science in conformity with the tempo demanded by socialist construction. Properly speaking, we ought to develop science at even greater speed. Science must indicate the road of our socialist construction in sufficiently long and wide perspective and not simply to allow us to try and grasp what has already been done. I am not so sure that bourgeois natural science is even capable of doing the latter. I think so, not only because I can see our practical socialist achievements, but also because I know the methodological, the philosophical basis on which bourgeois science rests. Let us examine the methodological, philosophical basis of this science.

This is what Engels writes: "The analysis of nature into its individual parts, the grouping of the different natural processes and natural objects in definite classes, the study of the internal anatomy of organic bodies in their manifold forms—

these were the fundamental conditions of the gigantic strides in our knowledge of nature which have been made during the last four hundred years. But this method of investigation has also left us as a legacy the habit of observing natural objects and natural processes in their isolation, detached from the whole vast interconnection of things; and therefore not in their motion, but in their repose; not as essentially changing, but as fixed constants; not in their life, but in their death. And when, as was the case with Bacon and Locke, this way of looking at things was transferred from natural science to philosophy, it produced the specific narrow-mindedness of last century, the metaphysical mode of thought."[1]

And we are invited to preserve this natural science; and on the basis of this natural science, the logic of which can only observe phenomena "not in their life, but in their death," we are invited to direct the creation of new biological forms also in socialist economy. This is the limit!

We understand, of course, that some natural scientists, and progressive biologists in particular, felt restricted in the framework of this philosophy, but they had no other, and only groped their way towards materialist dialectical logic which is capable of observing the living really in the living (Darwin, Timiryazev, and others).

Bourgeois natural science grew up on the basis of the metaphysical world outlook and is therefore accustomed to engrossing itself in particulars, forgetting the connection between the particular and the general. Eclectically linking the particulars obtained (sometimes very small and detailed) with artificial connecting ideas, with purely formalistic constructions, they thought it is possible to govern the world on the basis of these theories.

Two of Academician Lysenko's opponents—Zhebrak and Zavadovsky—demonstrated at this session their devotion to the commandments of bourgeois science. Both widely resort to technical means and researches—chemical, physiological, optical, and so forth, are able to see, stain, draw and count chromosomes, understand the subtleties of the biochemistry of

[1] F. Engels, *Herrn Eugen Dührings Umwälzung der Wissenschaft*, Moskau 1946, S. 23-24.

endocrine secretions, and so forth. They can do all this equally as well as any bourgeois scientist. And in no way superior to the bourgeois scientists, they lightheartedly link these well-known details with the ready-made, artificial schemes of bourgeois natural science—with a fictitious hereditary substance which is either totally divorced from the life of the organism (Professor A. R. Zhebrak), or is connected with the organism only by endocrinological threads (Academician B. M. Zavadovsky).

Academician Zavadovsky talked a lot about the numerous services he has rendered science. After his address I glanced through the two thick volumes of his *Zootechnical Endocrinology*. Let us see what problems Academician Zavadovsky dealt with, and what methods of assisting practical workers he proposed. Academician Zavadovsky is not a scientist who merely contemplates and describes the world. He is a practical scientist, and has always striven to gain the utmost benefit from his scientific achievements. By means of precise laboratory experiments he discovered that one of the endocrinological preparations that he was studying accelerates moulting among birds. As a practical scientist he could not calmly ignore this biological fact, and very zealously began to push this discovery into the poultry industry. He was aware that before you can roast a bird you have to pluck it, and this entails an expenditure of human labour. He decided to relieve mankind of this burden. After taking Academician Zavadovsky's preparation, a goose would forthwith "disrobe." Academician Zavadovsky did not wait for requests for such a device to come from the poultry industry, he took the initiative in propagating it; he began to push his "commodity" with no less zeal and with scarcely less skill than that displayed by private capitalists in pushing their wares. But the "commodity" did not sell, there was no demand for it, and it in no way affected the general line of development of our socialist livestock industry. The function of the Academy of Agricultural Sciences is to be the ideological leader of our socialist livestock industry in the effort to surmount the difficulties that stand in the way of its development, but a bald goose could be of no assistance in this.

Another "commodity" that Academician Zavadovsky discovered was a hormone that was supposed to "force eggs,"

that is to say, to cure barrenness, but this too did not sell, in spite of Academician Zavadovsky's determined efforts to push it.

Academician Zavadovsky wanted to assure us that the Michurin trend that Academician Lysenko is developing in the Soviet science of biology is a mechanistic trend. But what name would you give the idea of injecting a certain endocrinological preparation into an organism with the object of "forcing eggs out of it," or of "making its feathers fly," without caring what effect such an operation will have upon the organism, upon its heredity? Is that not mechanistic?

Academician Zavadovsky inherited this world outlook in its entirety from bourgeois natural science. Being profoundly convinced *a priori* that heredity can change under the influence of external conditions only among bacteria, he, not regarding himself as one of the latter, would not give up this bourgeois heritage.

The methodology that stands opposite to metaphysical methodology is that of dialectical materialism. "The dialectical method ... holds that no phenomenon in nature can be understood if taken by itself, isolated from surrounding phenomena, inasmuch as any phenomenon in any realm of nature may become meaningless to us if it is not considered in connection with the surrounding conditions, but divorced from them; and that, vice versa, any phenomenon can be understood and explained if considered in its inseparable connection with surrounding phenomena, as one conditioned by surrounding phenomena."[1]

The second specific feature of the new, Soviet trend in science, which is based on the materialist dialectical mode of thinking, is the connection between theory and practice. I have already shown in the case of Academician Zavadovsky what connection there is between the two in bourgeois natural science.

The interrelation that should exist between theory (science) and practice was excellently described by V. I. Lenin as far back as 1894 in his book *What the "Friends of the People" Are*. True, Lenin deals there not with the technique of produc-

[1] *История ВКП(б). Краткий курс*, стр. 101.

tion but with social science, but I think that his idea fully applies in describing the connections that should exist between the science of production and production itself.

Moreover, I am of the opinion that under Soviet conditions activity in production is social activity. I always try to impress upon my students that they must not regard themselves merely as technicians who know the details of production, but as public men working in the field of production.

Under socialist production, not only the director of an enterprise, but the whole staff, even the least skilled workers, must realize that when working at their job they are doing public work and not merely earning their means of existence.

Lenin wrote: "Of course, if it is presumed that the task of Socialists is to seek 'different (from the actual) paths of development' for the country, then, naturally, practical work becomes possible only when philosophical geniuses discover and indicate these 'different paths'; and the discovery and indication of these paths will, in turn, mark the close of theoretical work, and the beginning of the work of those who are to direct the 'fatherland' along the 'newly discovered' 'different paths.' "[1]

Do you not think, comrades, that these words apply directly to Professor Zhebrak's methodology? The American geneticists discovered and revealed to him what he thinks are new paths and, without troubling about methodological and theoretical problems, he calmly and confidently proceeds along that path which he thinks is beneficial for Soviet agronomics.

"The position is altogether different," continues Lenin, "when the task of the Socialists is understood to mean that they must be the ideological leaders of the proletariat in its actual struggle against actual and real enemies who stand across that *actual* path of social and economic development. In these circumstances, theoretical and practical work merge into one. . . ."[2]

The new paths of development of the livestock industry that Academician Zavadovsky saw was that of removing the feathers from geese by means of hormones, and the "new

[1] В. И. Ленин, *Сочинения,* том I, стр. 279.
[2] *Ibid.*

paths" that Professor Zhebrak saw for Soviet genetics were the discoveries of American philosophers.

A voice. Hear, hear!

D. A. Kislovsky. Both devoutly adhere to the conception of the interrelations between theory and practice by which bourgeois science and the Narodniks were guided. "The representatives of science think; the representatives of practice work," "respectfully allowing us to become engrossed in art and science, to give ourselves up to love and dreams".... Under the capitalist mode of production it is, of course, undesirable that "those who work with calloused hands" should think, because, if they began to think, they would understand that it was necessary to sweep away capitalist social relationships and create new, socialist relationships. But for your information, Professor Zhebrak and Academician Zavadovsky, I must say that even capitalist production cannot get along with workers who only have calloused hands. Capitalist production too needs workers who, in addition to calloused hands, have also clear heads. This is the fatal contradiction inherent in capitalism. Thanks to this fatal contradiction you and I have the good fortune to live and work in a socialist country.

And least of all in a socialist country, in the course of building Communism, should practical workers be regarded merely as "hands." Under socialist conditions, theory and practice must merge in a single whole.

Wherein lies the strength of T. D. Lysenko? In that he has become the ideological leader of the workers in socialist agriculture in their actual struggle against actual and real enemies who stand across the "actual path of social and economic development," no matter who these enemies may be, and no matter what toga of the "orthodox Darwinists" they may put on. T. D. Lysenko has come out at the head of our workers in socialist economy in their struggle against the elemental forces of nature which is often stinting in her gifts, in the struggle to alter nature in the direction we need.

T. D. Lysenko has succeeded in inspiring the masses with his idea of converting cultivated plants into the new and more productive varieties that our socialist economy needs by creating conditions that will lead to increased yields and to the alteration of the heredity of plants.

That is why we must by every means support and develop the theories of Michurin and Lysenko.

Our opponents have tried to convince us that T. D. Lysenko does not tolerate criticism. But I want to criticize him because I am profoundly convinced that he does tolerate real, businesslike criticism. He does not tolerate criticism in the fight against metaphysics. Here, indeed, there cannot be, and there must not be, any compromise.

A voice. Hear, hear! (*Applause.*)

D. A. Kislovsky. Once, in private conversation, I reproached Trofim Denisovich for misusing our zootechnical term "breed." Both he and many of his adherents often put the terms "breed" and "heredity" on a par. This use of the term "breed" cannot, of course, do any harm to the work of plant cultivation. But it is altogether different with livestock breeding, in which failure to distinguish between these two terms causes immense harm.

I assert most categorically, and everybody will agree with me, except perhaps the most stupid adherents of Johannsen's pure lines theory, that there are no two individuals in the organic world with the same heredity. Each individual has its own specific heredity. If we identify heredity with breed, we will have to say that we have as many breeds as we have heads of cattle, namely, millions.

Does such terminology assist the practical workers in our socialist livestock industry? I think it does not; it only hinders them in understanding the facts. By breed, zootechnicians mean a group of animals which are bound together in their evolution by the guiding, directing influence of zootechnical work.

This is not an artificial conception of breed, it is the result of a profound study of the facts connected with developing zootechnical practice.

I am aware that this is not mere carping over words, that the dispute is not merely about terminology.

This innocent dispute about terminology (or to put it more correctly, this failure to understand zootechnical terminology even by zootechnicians themselves) often gives rise to such things which I will refrain from qualifying but leave it to the audience itself to do so.

Comrade V. A. Shaumyan described the Kostroma breed of cows to us here, and one of the chief arguments he advanced to convince us that this was indeed a breed was the biological (physiological) fact that the blood pressure of the animals in the Karavayevo herd is much higher than that of any other breed.

Hence, according to Shaumyan, a biological factor—blood pressure—is decisive in determining breed and in distinguishing between breeds. I am not competent enough to appraise the significance of blood pressure among cattle and I am quite willing to learn from veterinaries who will explain to me the significance of this clinical, constitutional index for the organism of cattle.

I have had occasion to measure the blood pressure in my own organism. It was 180 mm. I informed Efim Fedotovich Liskun of my discovery. He, in turn, informed me that his blood pressure was 120 mm. Consequently, there is the same (or a similar) biological difference between me and Efim Fedotovich as there is between the Kostroma breed and ordinary cattle. According to what Comrade Shaumyan said, we two ought to be classed in different breeds. (*Laughter.*) I think that the absurdity of the purely biological conception of breed is clear.

The doctors gave me some explanation of the constitutional difference between me and Efim Fedotovich. They told me that a hypertonic (a man with a high blood pressure) has less chance of living to a ripe old age and more chance of a speedy, sudden death, without having to suffer the agony of death.

From this medical prognosis I draw certain conclusions which are no doubt the opposite of those which Efim Fedotovich can draw regarding himself. Efim Fedotovich need be in no hurry to express his opinion. His blood pressure is low. (*Laughter, animation.*)

I have a high blood pressure. I cannot put things off. Academician Perov from his seat gives me the medical advice not to get excited. You are right, Sergei Stepanovich, if one lives merely to prolong one's existence even in the form of vegetating then there is indeed no need to get excited. But I think that one cannot help getting excited. The subject we are discussing is too serious and important, and therefore one cannot remain calm.

Many zootechnicians, and still more laymen, have a totally wrong conception of breeds and of the formation of breeds.

A voice. Of regional distribution of breeds.

D. A. Kislovsky. Yes, and of the regional distribution of breeds. The two go together. There is a lot of harmful simplification in this matter.

Comrade Stalin teaches us that not a single phenomenon can be understood if it is examined separately from the conditions that determine it. What are the main determining conditions for the formation of breeds, what is the intrinsic nature of a breed, and are biologists who ignore practical zootechnics and their theoretical basis competent to judge in this question of breeds? I say they are not.

The biologist who treats practice with disdain, who treats zootechnicians in the same way as the "friends of the people" in the passage from Lenin I quoted treated practical workers, cannot understand the intrinsic nature of a breed. It must not be forgotten that Vladimir Ilyich Lenin, the greatest theoretician in world history, had good reason for giving the following formulation: "Practice is higher than (theoretical) knowledge, for it has the virtue not only of universality, but also of direct actuality."[1]

The essential condition which determines the nature of a breed is that a breed is the product of labour, and if this is ignored every conception of breed will inevitably be reduced to absurdity.

Many may think that I am wrong, but I must say that it grates on my ears when I hear people say: "The chief function of the Academy of Agricultural Sciences is to study agrobiology." In my opinion, the chief function of the Academy of Agricultural Sciences is to work out the theory of agricultural production under Socialism. This imposes a great obligation upon it. In this, agrobiology should serve only as one of the means of raising production to a higher level.

The chief and determining factor in the formation of breeds is people, their organized work. It is to be regretted that the assistant editor of *Pravda Ukraini* was the only speaker to deal with this point; most of the other speakers devoted their atten-

[1] В. И. Ленин, *Философские тетради*, 1947 г., стр. 185.

tion mainly to biology. Breeds are created by human labour. Breed is the product of human labour, of human will, of human thought. Breed is a value created by human labour. And whoever has read Marx knows how he ridiculed all those who think that the element of value in an article can be seen with the aid of chemistry, or of a microscope. Value is materialized labour. Technique itself is a progressive factor. Productively valuable labour must also inspire theoretical science. This labour is continuously providing it with new material for generalization and analysis.

In the course of my scientific work I have analyzed the practical work of pedigree stockbreeding with the object of discovering the technical methods of developing capitalist stockbreeding which had to come into conflict with capitalist relationships.

I am of the opinion that we cannot in our practical zootechnical work be guided by theories based solely on biology. The chief material for zootechnical theory should be the facts of practical zootechnical work subjected to a Marxian analysis.

The metaphysical zootechnicians cannot see breeds because of the animals, or else they want to see breeds in blood pressure as Shaumyan does, or in chromosomes, as Zhebrak does. One is as bad as the other. A breed is a large group of animals which have specific interrelations in their evolutionary progress. As far back as 1910, Pavel Nikolayevich Kuleshov, with his penetrating mind, defined the approximate dimensions a group must have to be called a breed. He realized that quantity must reach a certain level so as to produce a qualitative leap, so that a simple sum of individuals should become a breed. Soviet zootechnical science corroborates what Kuleshov said, but with a definite theoretical interpretation and a definite theoretical analysis.

But a breed is not an amorphous mixture of "several thousand splendid animals," a breed is something integral, a breed can be divided into parts. This division into qualitatively (hereditably) different parts enhances the evolutionary potentialities of the breed as an integral group. A breed, considered as a whole, possesses greater evolutionary potentialities than those obtained by the summation of the evolutionary potentialities in-

herent in every separate individual regarded isolatedly, outside of its interconnections.

Parts of breeds that are separated by enormous distances not only can but must be interconnected. Every ecologically different environment creates a qualitatively different adaptation. Some of the adaptations acquired by parts of breeds situated a long way from us, can be utilized in another place. This is achieved by the exchange of males between the parts.

Breeds, parts of which are developing under diverse ecological conditions and are interconnected, should undoubtedly have more evolutionary potentialities than if each of these parts were isolated from the others in their evolutionary development, if it were regarded as a separate breed.

A breed is of great social value. The problems of breeds, the formation of breeds, the distribution of a breed in space, the interconnection between the breeding and marketing departments of the livestock industry, and many other problems, must be approached as extremely important and complex problems of Soviet economics.

In his address, Academician Lysenko did not deal sufficiently with pedigree stockbreeding, and what he did say was not on a sufficiently high theoretical level. On the whole, however, he did undoubtedly correctly, and on the right political lines, concentrate attention on the extremely grave defects in the present state of biological science. I think that the discussion that has taken place here has clearly revealed that there are grave defects on the ideological front of science. These defects cannot be removed in a rush. Prolonged and systematic educational work is needed here to eradicate the survivals of bourgeois ideology in the minds of some Soviet people.

The function of training the socialist consciousness of our future agricultural experts rests upon our colleges and universities. When training Soviet agronomists and zootechnicians, the greatest attention must be devoted to this aspect of the matter.

We are faced with big and difficult tasks, but I am convinced that the Soviet public, the Soviet Government and our Bolshevik Party will, under the leadership of Comrade Stalin, surmount these difficulties as we have surmounted all the difficulties that have stood in the path of our social development. *(Applause.)*

Academician P. P. Lobanov. I call upon Academician I. F. Vasilenko.

Academician I. F. Vasilenko. Soviet agrobiology has laid down and continues to develop the theoretical foundations for increasing crop yields and the productivity of animal husbandry. There can be no dispute about the influence exerted by these ideas upon all agricultural sciences, including the theoretical conceptions concerning agricultural machinery. But whereas the influence of agrobiology upon the breeding of plants and animals is quite obvious, its influence upon and connection with the science of farming machines and the mechanization of agriculture require special, supplementary elucidation. This is the purpose of my speech.

Paramount importance attaches to the mechanization of agriculture as a means of obtaining higher yields and greater labour productivity. Lenin and Stalin, the great leaders of our people, teach us that maximum mechanization of labour is the principal line of development of socialist production.

Comrade Stalin said as early as 1931: "... mechanization of labour processes is for us the *new* and *decisive* factor, without which we shall be unable to maintain either our tempo or the new scale of production."[1] Rapid rates of mechanization of labour processes constitute one of the main conditions for the fulfilment of the postwar five-year plan in four years.

The mechanization of agriculture gives sweeping range to the development of agrobiological achievement. In our country this mechanization is progressing on a scientific basis. The founder of the science of agricultural machines is Academician V. P. Goryachkin, whose works are being published by the Lenin Academy of Agricultural Sciences of the U.S.S.R. Of seven volumes five have already appeared and two are in the press.

The followers of Academician V. P. Goryachkin, the workers of research institutes and colleges, including the Moscow and other institutes of the mechanization and electrification of agriculture, have elevated the science of agricultural machines and tractors to a high theoretical plane. Soviet science leads the

[1] И. Сталин, *Вопросы ленинизма*, 11-е изд., стр. 333.

world in questions pertaining to the theory underlying the building of agricultural machinery and tractors.

Undoubtedly technical disciplines form the foundation upon which the science of agricultural machinery rests. But it is a peculiar feature of the theory of agricultural machinery that it must be grounded not only on technology, as is the case with other departments of machine building, but also on Soviet agrobiology. And indeed, soils, plants and grains, as Academician T. D. Lysenko remarked in his address, are living bodies. A study of such bodies transcends the scope of technical knowledge. Hence only a profound interconnection between the science of agricultural machinery and agrobiology will make possible the establishment of a genuine theory governing these machines, a theory that will illuminate the course of development of the designing of machines, and of the methods of their exploitation. The direction taken by science is a question of world outlook. Our world outlook is dialectical materialism, the theory of which was created by Marx, Engels, Lenin and Stalin. The classics of Marxism indicate the direction our young discipline, still in its developmental stage, ought to take. From the classification of sciences made by Engels on the basis of the dialectical-materialist conception of nature and set forth in his work, *The Dialectics of Nature,* it followed that the further development of new knowledge would proceed along the common borders of the principal existing sciences. As we see, this remarkable prediction has really come true. From the fundamental sciences—physics and chemistry—the science of physical chemistry was created by Lomonosov, Mendeleyev, Butlerov and other scientists. Biology and chemistry gave rise to biochemistry, based on the works of K. A. Timiryazev, Academicians Bach and Oparin, and other scientists. Agrobiology evolved from agronomy and biology. In some cases it proved necessary to unite even three sciences, as is instanced by biogeochemistry, a product of the creative minds of two of our Academicians, Vernadsky and Fersman.

Our science must be built up on agrobiology and technology as its foundation. It must derive its support, on the one hand, from the works of Timiryazev, Dokuchayev, Williams, Kostychev, Michurin and Lysenko and, on the other, of Academician Goryachkin and his followers. This was the point of view adopt-

ed in the new curricula covering this branch of learning in the Molotov Institute of the Mechanization and Electrification of Agriculture at Moscow. A month ago the all-Union conference of members of the departments of the leading colleges of agricultural mechanization approved this curriculum for all our university faculties and higher educational institutions teaching this specialty.

Let us examine some concrete cases taken from practice and see to what extent the line chosen by us is correct. Let us see what demands the science of agrobiology makes upon the designers of farming machines and ascertain wherein these machines, our native product, differ from machines designed for the same purpose but produced in the capitalist countries.

Let us take up first the important group of soil-preparation machinery. Soils are ploughed in all countries of the world and the object of ploughing is to turn the soil and make it friable.

Soviet agrobiology presents additional demands—to go beyond the mere turning of the furrow slice and have the upper layers, whose structure has been destroyed, lie at the bottom of the furrow together with the weeds; this will improve the structure of the soil and kill the weeds. It was just for this purpose that the skim coulter was created.

Our plough-bottoms are designed according to the theory propounded by Academician Goryachkin. At present we are manufacturing only ploughs provided with skim coulters and the agrotechnical quality of our plough is appraised very highly.

The ploughing implements produced in capitalist countries are of a quite different order. Upward of two thousand types of ploughs are in use throughout the world. This diversity of types has no scientific justification. It is merely the result of competition between different firms trying to dispose of their different types of ploughs. This confusion has led to the establishment in the U.S.A. of a governmental commission consisting of agronomists, engineers and mathematicians who were to examine into the designing of ploughs. Good results were achieved by the commission with regard to construction and technology; nevertheless the vices of the capitalist system prevent the use of skim coulters in the U.S.A.; in consequence enormous areas are overrun by weeds.

In the U.S.A. agronomy is not linked up with biology. There there is no agrobiology. As for general biology, this science is completely divorced from the interests of the American people. Reactionary in essence, it has been and still is unable to afford any assistance in the cultivation of the soil. Agronomists and biologists in the United States have permitted extravagant waste of soil fertility during the process of ploughing. This exploitive tendency can be observed to an even greater extent in the case of grain combines. Harvester combines in the U.S.A. cut the cornstalks high, only the grain is gathered and the straw is scattered over the field by a straw spreader. Great quantities of weed seeds are spread together with the chaff and straw. They have no mulching there.

As a result the United States, it transpires, occupies one of the first places in the world for potential weediness of soil, as is stated by S. A. Kott, a scientific staff worker of the All-Union Institute of Plant Industry in his work, *The Biological Peculiarities of Weed Plants, and Weed Control* (Selkhozgiz, 1947).

Crop yields are not high in the U.S.A., nor are they increasing. Even in the American press warnings have been sounded that soils are extremely exhausted, that farmers cannot make out well on exhausted land and are being ruined. Thus, for instance, Clinton Anderson, Secretary of the U. S. Department of Agriculture, wrote in *America,* No. 8, 1946, that the most urgent problem facing the farmer was that of restoring soil fertility; that he could not be expected to raise a good crop on exhausted land and that the war had compelled him to work the land to the very limit. Further on he added that soil fertility must be conserved, and that despite growing requirements the demands made on the land must not be permitted to conflict with the demands of science. But, as the examples given show, they already conflict completely with the demands of science.

The biologists of that country have not come to the aid of the people. On the contrary, they passively observe how the profit-hunting distributors of agricultural machinery teach the farmers outrageously wasteful methods of tillage, and how this enormous waste is already reducing a considerable number of farmers to ruin. A science that spurns the interests of its people, is ignorant of them, betrays that people.

We use grain combines in our agriculture. There was a time

when we could not rid the combine of its defects, at least not at once.

Now Soviet agrobiology has pointed out to us the way to high-grade agrotechnique, which consists in cutting cornstalks low, gathering in the straw, chaff and weed seeds, the practice of mulching and of deep ploughing with skim coulters. Ploughs provided with skim coulters cannot be used with success if the combines leave the stubble high. When the combine leaves high stubbles and the field is not sufficiently clear of straw, skim coulters get clogged. Work has to be stopped to clean them and since the tractor driver cannot for this purpose shut off the motor of the tractor and then start it again, the motor runs idle. This results in an over-consumption of fuel as high as 20-25%.

A combine should ensure low cut of cornstalks. It is sometimes thought that low cutting is a matter of the Ministry of Agriculture issuing instructions. It is believed that as soon as such instructions are received all combine operators will be cutting low. That is not true, for a combine operator will cut low only when the combine is able to put through efficiently large quantities of straw, i.e., when the efficiency of the thresher and the power of the combine motor make that possible. This problem has been solved in the case of trailer combines by the production of the Stalinets-6, which can handle 25% more vegetative mass than the prewar combine of the same span. In a self-propelled combine the thresher is wider, the straw shaker longer and separator productivity greater. Thus our new combines are better equipped for low cutting.

To stop combines from spreading weeds we have provided them with trailcar stackers for straw and chaff. These stackers form and eject big loads 15 cu. m. in volume and 300-400 kg. in weight. The stacker does everything automatically; all the workers have to do is straighten out the straw in its corners. Where there is a stacker no weed seeds are thrown about, the field is quickly cleared of straw and can be mulched right after the harvesting is over.

It must be said that collective farmers, particularly in the South, have begun on their own initiative to stack straw after the combine. They build primitive straw stackers mounted on the combine or trailed. But this crude method of work is very

laborious and besides creates difficulties in the transportation of the stacks. These handicaps make it impossible for the collective farmers to collect the straw and chaff in full, they cannot clear the fields of straw in time and are late with their mulching and ploughing.

All the trailer combines we are now putting out are equipped with stackers. Yesterday's *Pravda* informed us that the 5,000th Stalinets-6 combine left the conveyor at the Rostselmash Plant. It must be borne in mind that production of stackers hampers the output of combines as their manufacture requires a lot of factory space on account of their bulkiness. However, we demonstrated the necessity of stackers for combines on agrobiological grounds and had them included in combine production.

With low-cutting combines, stackers, sweep rakes, disk tillers and ploughs with skim coulters we are completely mechanizing all the operations of harvesting and autumn soil preparation in accordance with Academician Williams' theory.

For the mastering of this new technique, harvesting and soil preparation work was carried on in 1947 in Krasnodar Territory, Omsk Region and Altai Territory on an area of 10,000 ha. in such a manner that the whole cycle of operations, namely, harvesting with low cutting of cornstalks, straw and chaff gathering into big stacks, transport of the stacks to the ricks, mulching and deep ploughing with ploughs provided with skim coulters, was gone through in full. During these tests harvesting was proceeding on an area of 25,000 ha., on which the combines deposited more than 100,000 stacks, which shows the absolute dependability of the stackers. The collective farmers were entirely satisfied with our work. (*Exhibits.*) The photograph shows the progress of the work and its results. The TASS photographer took the pictures on a wheat sector of the Stalin State Farm in Krasnodar Territory whose grain yield was almost 27 c. per ha. Academician I. V. Yakushkin has shown you here a sample of the wheat harvested. The photographs show that in this field all work was done in accordance with the requirements of agrobiology.

While producing machines for combine harvesting we are at the same time already making partial preparations for the solution of another highly important problem, i.e., mechanization in grass growing. The stackers, pickups and sweep rakes

for straw are also good for haymaking. Our factories manufacture good horse-drawn, mechanically-drawn and suspension tractor hay mowers. In addition there has been produced a five-beam self-propelled mower with a 10 m. span. It is of original Soviet design, very manageable and can move along roads at a speed of 10-18 km. an hour. It is easily collapsible for transportation and readily negotiates all bridges and dirt roads. There remains unsolved the question of hulling grass seeds and of scarifiers for scratching hard-coated seeds. A scarifier of original Soviet design is undergoing final tests. As for clover hullers, there are such machines but we do not consider them entirely satisfactory and are working on a plan to use hammer mills instead.

The production of all these machines opens up vast prospects for the application of Williams' doctrine to our fields. The technical means are being provided wherewith our collective farmers may master the achievements of the science of agrobiology. Indubitably this will lead to a considerable increase in crop yields and to an abundance of agricultural produce. This is the great service that our agrobiological science is rendering the people. A science like Soviet Michurinist agrobiology, which protects the interests of the people, is the handmaid of the people, must be given our utmost support. We must allow no one to detract from the authority of those who have inspired and organized this science.

Let us take another illustration: machines for the mechanization of stockbreeding farms. Capitalist technique has brought forth milking machines. They are two-beat machines. The machine produces the beat of the squeezing of the cow's teat and the beat of the sucking of the milk. It was found that these machines are harmless to cows with sound udders and only if the machines are carefully attended to. If a milking machine is used on a cow suffering from mastitis in a latent form the ailment will become more severe. Our investigators, after studying the physiology of cows, have produced a three-beat milker in which the beats of the squeezing, sucking and resting of the teat regularly succeed each other. The process of milking with the aid of the third beat, the resting of the teat, during which period its blood circulation is restored, is, taken together with the first two beats, the equivalent of the process of

a calf sucking the cow's milk. Tests have proved the great merits of the three-beat milking machine. It represents an achievement in the application of biological requirements to the production of this type of machinery.

However, we are not always successful in meeting agrobiological requirements. We lag behind in such matters as the cleaning and sorting of grains and seeds both in regard to the efficiency of these machines and the production of machines separating grain mixtures in accordance with the latest methods. Agrobiology has demonstrated that if seeds of a high specific gravity are used crop yields sometimes rise as much as 20%. And this has been proved in quite a number of cultures. However, the laboratory method of separating grain mixtures by their specific gravities, namely, the method of immersion in solutions, is not practicable on a large scale. A machine has to be constructed that will perform this separation by a dry method. But such a machine has not yet been built. Here we do not keep pace with agrobiology, but the bare fact that this clear and concrete task has been posed is an earnest that such a machine will be built.

The examples given are convincing evidence of the fact that the course we have chosen for the development of our science is correct and that this course will cause our science to prosper. Our agricultural machines and tractors already include a number of designs that have been awarded Stalin Prizes. Among them is the Pustygin and Ivanov self-propelled combine, which is now being redesigned to include the stacking of straw, chaff and weed seeds.

I. V. Yakushkin. In the meantime it scatters them.

I. F. Vasilenko. Yes. It is still scattering them, but take the trailer Stalinets-6, a picture of which I am demonstrating here. It no longer scatters weed seeds. By government decree it is now made only with a stacker and when it is shipped to the points of local distribution the latter are supplied at the same time with stubble ploughs and ploughs provided with skim coulters. Thus the farms receive all they need to mechanize the operations called for by Williams' teaching concerning autumn ploughing.

We also have a flax combine designed by Shlykov, Moiseyev and Mayat. This combine is based on the system of a flax pull-

er which has already stood the test of extensive practical use and has proved very dependable. The Gogolyev grain dryer, also awarded a Stalin Prize, is of such design that it can easily be made on collective farms. Collective farmers produce them in great quantities, particularly in the Urals, and thus prevent much grain from spoiling. The designers of the Kirovets KD-35 caterpillar Diesel tractor are also Stalin Prize recipients.

In addition there are a number of other new farm machines for which blueprints have been completed and production started. Among those worthy of note is the above-mentioned wide-spanned self-propelled hay mower designed by Volkov, the beet-harvesting combine designed by the late Academician Sivachenko and subsequently elaborated upon by Pavlov and Gerasimov. Then there is the three-beat Korolyov milking-machine of which I have already spoken, the Nastenko potato planter, suspension tractor implements and a number of other products of engineering.

The inclusion of the sciences underlying agricultural machines and tractors, and the electrification of agriculture in the general system of sciences integrated by the Lenin Academy of Agricultural Sciences of the U.S.S.R. is of great importance for their further development.

In reliance upon Michurinist agrobiology we shall continue to raise the theoretical level of our science. We take legitimate pride in and shall safeguard the priority of Soviet science in the theory of agricultural machine and tractor construction. At the same time we shall impart to our science the ability to exercise practical direction in the solution of our most urgent problems —raising crop yields, increasing labour productivity, creating an abundance of products, further improvement in the well-being of our people and strengthening the might of our Soviet Country. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician A. N. Kostyakov.

Academician A. N. Kostyakov. In his address T. D. Lysenko has splendidly developed the fundamental ideas and principles of Michurin's and Williams' teachings and has shown their immense importance to all branches of biological science.

I should like to dwell briefly on the importance of these ideas in the branch of science in which I am working, namely, agricultural land improvement, that is to say, a science directly concerned with the questions of remedying unfavourable natural conditions for our agriculture.

V. R. Williams dealt with questions of land improvement throughout his scientific career. He may justly be regarded as the founder of our land-improvement science, and his works provide a wealth of material directly contributing to its development. Our entire science and practice of land reclamation ought to be based on Williams' teachings.

I shall note the major tasks with which we are confronted in the light of these teachings. In his brilliant works Williams developed and explained the law of the irreplaceability or equivalency of the factors of plant life, and demonstrated the significance of this law in agriculture. What does this law contribute to the sphere of land improvement? It touches on fundamentals, on points of principle. It tells us that to attend to the water factor alone in land improvement is not enough, that simultaneously with changes effected in the water conditions, and in conformity with these changes—quantitatively and qualitatively—attention must be paid to the changing of other factors, namely, nourishment, temperature, etc.

Williams says: "We distinguish two elements of soil fertility—water and the food that plants use. Both elements of fertility—water and food—are absolutely indispensable to the plant, and neither can be replaced by anything else."

It follows from this that the use of water in our land-reclamation systems—in fact, the entire operation of such systems—must be linked with agrotechny, with the state of soil fertility and the cultivation of agricultural crops. In every land-reclamation project it is essential to provide not only for measures to regulate the water supply, but also for agricultural measures—crop rotations, fertilizing, tilling, etc.—all inseverably connected and forming a single complex. "In irrigation, as in all branches of agricultural production," wrote Williams, "we can achieve progress and prospects of improvement only by directly influencing all the factors, of the complex—and not of the sum—of which the given branch is made up."

Reclamation measures must be exactly calculated to correspond to a definite type of economic utilization of the reclaimed land in accordance with the plans of the state. Thus the law of the equivalency of factors in agriculture sets quite definite tasks both with regard to the operation and with regard to the drawing up of land-reclamation projects.

Unless this law is observed, we get a discrepancy between the construction and the operation of land-reclamation systems, a lowering of yields and a drop in the efficiency of reclamation measures.

It must be admitted, however, that unfortunately not always and not everywhere are the proper conclusions drawn from this law, and this leads to the consequences which I have pointed out.

Land-reclamation measures radically change the water regimen of the soil and thereby most markedly influence the nature of the processes taking place in the soil. By these measures we intervene in the natural soil-formation process, we modify and change it. From the point of view of farming requirements these changes may be beneficial or they may have a negative effect. It is therefore necessary so to direct land-reclamation measures as to ensure that all the processes and all the changes which take place in the soil should be of benefit to our economy and meet its requirements. Land-reclamation schemes can neither be properly designed nor efficiently operated without an analysis of, and consideration for, the dynamics of the soil-formation processes and the changes in these processes brought about by the reclamation measures. This is necessary in order that we may plan the direction, technical details and extent of the land-reclamation measures. The latter must make it possible to regulate not only the dynamics of the water regimen, but the aeration and nourishment of the soil, so as to increase its fertility, obtain high yields and advance socialist farming.

Williams' teachings provide the key to the correct solution of these problems.

Very important points to be considered in land reclamation are suggested by Williams in those of his works which deal with the formation of the structure of soils and the travopolye system of farming. His teachings in these fields are of cardinal

importance for all questions of land reclamation. But they are particularly important to questions of irrigation, for such questions are encountered in work in arid steppes, where the soils are structureless.

"The soils of the steppe areas," wrote Williams, "can be rationally used in the case of irrigated culture only within the travopolye system of farming."

A water-stable crumble structure of soils, produced by travopolye farming, is highly essential for securing a proper water supply and salts content in the soil.

"The deep subsoil waters," wrote Williams, "must be isolated from the surface water regimen of the arable layer; they should be separated along the entire dividing surface, so that the irrigated top layer is completely isolated from the layer below it. This is only possible by lending the arable layer a water-stable crumble structure, which can be achieved by introducing the travopolye system of farming."

In another of his works Williams wrote: "The conversion of steppe soils into structural ones will not only ensure them against renewed secondary salinization; the acquired structure will enable these soils to absorb the entire amount of atmospheric precipitation and protect the water supply produced by such precipitation against being wasted, assuring its expenditure only for the requirements of cultivated plants. The amount of irrigation water will therefore immediately be reduced."

On irrigated land, the travopolye system of farming, by rebuilding the soil, ensures its structure. When the irrigated soil has a crumble structure, it requires greatly reduced irrigation and watering rates—the watering regimen changes and becomes less exacting. Besides, only when the soil has a crumble structure is it possible to regulate the concentration of soil solutions required by plants and thus to prevent both salinization and swampiness. But the significance of the travopolye system on irrigated land does not end there. A crumble structure of the soil, ensuring as it does its high fertility, makes it possible to obtain the maximum effect both from irrigation and from other agrotechnical measures connected with it. Here is what Williams wrote concerning this question:

"In a soil left to the natural development of the processes which take place in it the two elements of soil fertility inevitably

conflict. When such a soil contains the maximum amount of water needed for the plants, all the elements of the plant food in it pass into an unassimilable state. On the other hand, when the entire amount of the plant food elements is in assimilable forms, there is no water in such a soil.... On such a soil [structureless—*A. K.*] *any* plant-growing measure is restricted to the possibility of reaching, as a *maximum,* only 50% of its potential effectiveness."

In soils with a crumble structure there is no conflict between water and food substances. Such a soil provides the conditions for most fully bringing out its effective fertility.

"In a soil with a crumble structure *both elements of its fertility*—water and the assimilable substances of the nutriment required by green plants—can be contained *simultaneously* in the maximum quantities needed for the plants," wrote Williams.

Only on such a soil will each agricultural measure—irrigation, fertilization, use of selected seeds, vernalization, etc.—manifest an active effectiveness equal to its potential effectiveness; in other words, the expenditure of labour and means will reach a 100% efficacy.

At the same time the introduction of the travopolye system presents definite demands to the regimen and technique of irrigation, which must be such as will best meet the conditions of structural soils and will preserve their structure.

V. R. Williams maintained that it was necessary radically to reconstruct the technique of irrigation and watering systems. He was in favour of sprinkling. Here, for example, is what he wrote on this question in regard to the conditions in the Trans-Volga areas:

"The organization of irrigation in the Trans-Volga areas expressly by means of sprinkling represents the only correct method of coping with drought, of the struggle for stable harvests."

At present, when large-scale irrigation work is in progress in the central areas of our country, these words of Williams' are of special importance.

But Williams regarded sprinkling alone as insufficient for the reconstruction of irrigation technique. He held that the entire conducting network of irrigation canals should not be open but covered. He insisted that irrigation technique should

be thoroughly revised and based on new, rational principles. At present, in connection with the development of our agriculture, we are coming close to the realization of this task. Meanwhile we must work to raise the utility coefficient of our irrigation systems, carry out necessary measures to reduce the loss of water in canals, to utilize it more efficiently by reducing the amount of water wasted in irrigation.

The building up of a crumble structure of irrigated soils by means of the travopolye system of farming, rational and efficient expenditure of irrigation water and a proper irrigation technique which should guarantee against the wastage of water and the deterioration of the soil structure—those are the ways to prevent salinization of irrigated soils which follow from Williams' teachings.

In addition to keeping up the structure of irrigated soils, Williams, enlarging on Dokuchayev's ideas, considered it essential to have forest shelter belts on irrigated tracts. He attached very great importance to such shelter belts.

The system of measures involved in the formation of shelter belts should make it possible to improve not only the irrigation conditions, but the climatic conditions of the irrigated territory.

In connection with the question of the travopolye system of farming and the formation of shelter belts, Williams advanced the idea of planning the distribution of the main types of land (forests, fields, pastures) and the main branches of agriculture throughout the country to fit the main elements of the relief (watershed, slope, alluvial plain). Elaborating Dokuchayev's ideas, Williams spoke of the necessity of widespread integration of hydrological, land reclamation, forestry and agricultural measures into complexes.

Let me recall Vasili Robertovich Williams' words: "The problem of increasing and stabilizing harvest yields is the paramount problem of agricultural production in the Soviet Union and may be reduced to the problem of stabilizing the water regimen of the country's soils." ... "This must be achieved by systematic integration of forestry, agrotechnical (travopolye farming) and land-reclamation measures into complexes for regulating the water supply in the soils and increasing crop

yields, which should be distributed in conformity with geomorphological conditions and the types of the relief."

These ideas and propositions put on the order of the day the task of properly organizing our country's agriculture, water supply and forestry. The measures for coping with drought, for preventing soil erosion, for building up structural soils, and for a rational utilization of water resources confront us with the task of changing not only the nature of soils, but the nature of the hydrological and even climatic conditions of various sections of the country.

This task can be accomplished only in our country with its socialist agriculture and planned organization of the national economy as a whole.

We all know that the ideas of Dokuchayev and Williams had no chance of being materialized before the Revolution. It is only now, when we have a socialist economy, that these ideas are being applied in practice.

Such are the fundamental propositions emanating from Williams' teaching.

We must make still deeper, fuller and wider use of this teaching, develop it further and implant it both in the theory and in the practice of agricultural land improvement.

The underlying principle of Williams', as of Michurin's, teaching is the remaking of nature. Michurin's and Williams' teachings provide us with the methods of remaking the nature of plants and animals, the nature of soils, and of hydrological and climatic conditions.

By applying these teachings and developing them, our Soviet science will score new big successes in remaking nature and will thus make a great contribution to the building of Communism in our country. (*Applause.*)

Academician **V. P. Mosolov.** Academician P. P. Lobanov has the floor. (*Loud applause.*)

Academician **P. P. Lobanov.** Comrades, the question of the situation in biological science, which we are discussing at this session, is one of utmost importance for the further progress both of the biological and agricultural sciences and of socialist agriculture with which these sciences are inseverably connected.

It is incumbent upon our agricultural science, which is developing on the basis of the all-conquering teachings of Marx, Engels, Lenin and Stalin, to render daily assistance in the struggle for the further progress of our agricultural production (*applause*), for the accomplishment of the tasks which our Party, the Soviet Government and Comrade Stalin personally set before socialist farming.

Such assistance can only be rendered by an advanced agrobiological science, a science equal to the grand tasks of the socialist system of agriculture and possessing the inexhaustible forces and opportunities offered by the collective-farm system.

We have such an advanced agrobiological science in the shape of the Michurin Soviet agrobiology, whose foundations were laid by Michurin and Williams and which is being creatively developed by Academician T. D. Lysenko.

Michurin's teaching on how to alter the nature of plants is organically bound up with Williams' teachings on the process of soil formation, on man's active influence upon the soil, on the restoration of soil fertility and the achievement of higher harvest yields.

The Michurin science has ushered in a new, higher stage in the development of materialist biology, it enables us not only to explain the phenomena of life, but to change the life of plants and animals in a direction needed and useful to man.

In the Michurin science we have a generalization and a powerful development of all the best that has been accumulated by science and practical experience in past ages.

Our scientists, the true representatives of this science, have a big share in the work of eliminating the age-old backwardness of agriculture; they are helping to strengthen the collective farms, state farms and machine and tractor stations organizationally and economically and are making a real contribution to the postwar restoration and further development of agriculture.

But we cannot rest content with the successes so far achieved in the development of agriculture and agricultural science. These successes should be regarded only as a material requisite for a further mighty progress of field farming, animal breeding and other branches of agriculture, as a basis for the further

development of scientific thought. We possess vast and inexhaustible resources for a further rise in the output of agricultural produce, for higher productivity of agricultural labour. One need but scan the results of the work of advanced collective farms, machine and tractor stations, state farms and innovators in agriculture to realize how great are these resources. Advanced farms obtain high harvest yields on large areas, increase livestock production, and turn out high quality produce at a low cost. The successes scored by the advanced farms are accessible to all.

By applying the experience of these farms we can and should raise the efficiency of our entire agriculture.

The Gigant State Farm, known to the whole of our country and throughout the world, has been eminently successful in obtaining high harvest yields under the extremely unfavourable conditions of the Salsk steppes even in droughty years. It owes its success to the fact that it has introduced the travopolye system of farming and that it applies the teachings of Williams, Michurin and Lysenko.

The collective farms served by the Deminskaya Machine and Tractor Station are showing examples of good work for high yields in the Stalingrad steppes, where only recently the heroic soldiers of the Soviet Army were putting Hitler's army to the rout.

The collective farms served by the Williams Machine and Tractor Station are growing bumper crops in the Orenburg steppes.

On the basis of progressive biological science and through the self-devoted efforts of Kostroma collective farmers and workers of the Karavayevo Sovkhoz, a new cattle breed, one of the best in the world, has been produced—the Kostroma cow, with its very high milk yield.

We cannot agree with the opinion of the proponents of so-called formal genetics who maintain that the Kostroma cows had long carried within them genes which have only now become evident. If we were to be guided by such opinions and sit with folded arms, the formation of plant varieties and breeds of animals may indeed decline, as Academician Schmalhausen is prophesying.

Thanks to the everyday assistance and the concern of the

Soviet State for the strengthening and advancement of the collective-farm system, our agriculture has achieved substantial success in the postwar period. The gross harvest of cereals in 1947 was 58% higher than in the preceding year. The yield reached the prewar level, and state grain purchases totalled nearly as much as in the best prewar years.

This year our agriculture has made still further progress.

The spring sowing was carried out successfully; the cultivated area expanded by more than 11,000,000 hectares as compared with the year 1947, and this included an expansion by 5,500,000 hectares of the area sown to spring wheat.

The plan for the planting of cotton, sunflower and other industrial and oil crops has been fulfilled and exceeded, and the areas planted to sugar beet, potatoes and forage crops have increased.

The collective farms and state farms are gathering a good harvest. Notable success has been achieved in livestock breeding. In the first half of 1948 the cattle population increased by 15%, the number of sheep and goats by 34%, and the number of pigs by 29%. There is a definite improvement in horse breeding, and in the past six months the number of horses has increased by 13%, or at twice the rate of increase of last year. Livestock productivity and the output for the market have also increased.

All this testifies over and over again to the advantages of the socialist economic system, to the inexhaustible capacities and opportunities of the collective-farm system.

All this shows that real conditions have been created in agriculture for a further great rise in production, for a higher level of agriculture, and for the development of livestock breeding. Opportunities for increased production are secured by the annually growing power capacity in agriculture, by the growing number and further consolidation of machine and tractor stations, by the increase in the numbers of agricultural forces and the unswerving determination of the Soviet people constantly to develop their socialist farming, the most advanced system of farming in the world.

Our wonderful forces of outstanding masters at obtaining high crop yields, experts and organizers of socialist farming, managers of collective farms, machine and tractor stations and

state farms, rely in their practical work on the advanced Michurin science.

The socialist emulation among the broad masses of collective farmers and workers on machine and tractor stations and state farms, the patriotism of the Soviet peasants, their love for their country and their noble ambition to supply it with ever larger quantities of grain and other farm produce are the motive forces in the struggle for the further progress of agriculture.

A major condition for success in the postwar progress of our agriculture is the development of our advanced agronomic science and the application of its achievements in agricultural production.

The teachings of Michurin and Williams are constantly developing, and a great deal of credit for this is due to Academician Lysenko. He has skilfully combined Timiryazev's and Michurin's teachings on form development and on alteration of the nature of plants and animals with the teachings of Dokuchayev, Kostychev and Williams on soil formation and the methods of increasing soil fertility, providing the collective farms and state farms with weapons in their struggle for a steady increase of crop yields and for the advancement of scientific agriculture in our country.

Academician Lysenko and his followers do not confine themselves to words. They have been developing the finest traditions of our agrobiological science in practice and already have considerable achievements to their credit in this respect.

The ranks of the Michurinists—advanced Soviet biologists and agronomists—are constantly swelling. The notable successes of our Soviet plant breeders, such as Yakovlev, Kanash, Lukyanenko, Zhdanov, Ushakova, and of our animal breeders, as, for example, Yudin, Greben, Shteiman, Balmont, Filyansky, Vasiliev, and many others, serve as a guarantee that, far from declining, the formation of breeds and varieties in our country will keep developing on an ever larger scale. (*Applause.*)

The achievements of the true followers of Michurin and Lysenko have been demonstrated here by a number of speakers in the course of the discussion. They have told us of the principal results of the work of our foremost scientific research institutions and individual scientific and practical workers.

These results not only confirm the scientific truth and practical effectiveness of the major propositions of Michurin genetics, as the progressive trend in Soviet biology, but represent a tangible contribution to the further development of this trend.

Unlike the followers of the Michurin trend, the anti-Michurinists have come to this session without any real, tangible, practical results, with mere promises of "great" discoveries to come—promises which we have heard many times before.

This in itself is indubitable evidence of the fallaciousness of their theory.

The main tasks we are facing in agriculture today are: to increase crop yields and the total output of farm produce to the utmost, to improve farming methods by applying the achievements of advanced agronomic science, to develop stockbreeding as fully as possible and to increase its productivity, further to strengthen the collective farms organizationally and economically, and to develop them more fully.

In the light of these tasks we cannot say that the rate of crop-yield increases we have so far achieved, even though considerable, is satisfactory. In the light of our immediate tasks we cannot say that the tempo and methods hitherto employed by our scientists in dealing with problems of agriculture are satisfactory. The time has come when we must achieve a radical increase of crop yields in our country. It is the task of scientists to work out a system of measures which will help all collective farms and state farms to increase crop yields at a rate many times faster than known heretofore, and which will ensure a further rapid rise in labour efficiency. This is imperative if we are to meet the increased demands our country presents to agriculture today.

We must work out for the various zones fitting methods of achieving a radical increase in the yields of grain, industrial and other crops so as to ensure a considerably larger output of farm produce.

In stockbreeding the task is to achieve in a short space of time a sharp increase in the population of livestock of all types, raise their productivity and create a stable forage base for the growing livestock industry. In stockbreeding, the rate of development and the methods of work we have had in the past fall

short of the tasks facing us today. The achievements of zootechnical and veterinary science must be widely applied in practice, and every encouragement must be given to daring innovations in the science and practice of stockbreeding.

One of the urgent tasks facing agriculture is that of developing combined mechanization. We must have a system of machines to cope with the new and big tasks. Besides making fuller use of the available machines, we must create new types of tractors and tillage and harvesting machines which will meet the heightened demands of agriculture and ensure a considerable rise in the productivity of labour.

There is a vast amount of work to be accomplished in the sphere of electrification in agriculture. Many collective farms, and even whole districts and regions, are widely using electricity in agriculture. But that is only the beginning of a great effort. The task today is to make wider and fuller use of local rivers and other power resources for building an extensive network of rural electric power plants, so that electricity should not only become part and parcel of collective-farm life, but be widely applied in agricultural production.

In the sphere of water supply there is, in addition to the further development of irrigation construction in the Central Asian republics and the Transcaucasus, a huge amount of work to be done to keep sections of the Ukraine, the Central Black-Earth regions, North Caucasus and the Volga areas supplied with water.

All scientific forces must be enlisted for the practical prosecution of the tasks in the field of water supply. We must organize a study of the questions of how to obtain bumper crops on irrigated land, and of how to bring under cultivation all areas at present unused on account of salinization or swampiness.

Agricultural practice demands a considerable augmentation of scientific research in the sphere of the economics and organization of farming. It is obvious that the practical experience of advanced agriculturists must be better generalized.

In view of the bigger tasks which face agriculture, we cannot confine ourselves to the application of separate agrotechnical methods. Scientific agrotechnical measures must be applied in whole complexes and on a large scale. A powerful

means for a radical improvement in agriculture, for putting farming on a scientific basis, is to go boldly ahead with the practical application of the travopolye system of farming as elaborated by the most eminent representatives of Russian agronomic science—Kostychev, Dokuchayev and Williams. What has been a century-old dream of progressive Russian agronomists must now become a concrete program of action and translated before long into living reality.

Socialist agriculture has at its command everything necessary to make this progressive system of farming a reality.

It is our duty to help the collective farms, machine and tractor stations and state farms to apply the travopolye system in practice. At the same time we must work to concretize still further the details of the system to fit it for the various sections of our country.

The country expects of Soviet scientists such work as will help obtain high and stable crop yields and a high output of stockbreeding and other branches of agriculture in all the zones of the Soviet Union.

By making proper use of the achievements of advanced science, collective farmers, men and women, Heroes of Socialist Labour, Stalin Prize laureates, excellent bumper crop producers, and organizers of collective-farm production, have become outstanding agriculturists applying progressive science in practice. The experience of these advanced agriculturists and their achievements enrich our agricultural science, lend it new strength, open up new vistas before it.

But science must be ahead of practice.

Comrade Stalin said back in 1929, at a conference of Marxist students of the agrarian question: "... theoretical work must not only keep pace with practical work but must keep ahead of it and equip our practical workers for their fight for the victory of Socialism."[1]

Our science is entirely at the service of the Soviet state and the Soviet people.

The Soviet people demand of the scientists who have risen from their midst selfless devotion to the great ideas and cause of the Party of Lenin and Stalin, they demand of science that

[1] И. В. Сталин, *Вопросы ленинизма*, стр 275.

it should develop in a direction which will enable it best to serve the vital interests of socialist society, and that the scientific forces of various branches of knowledge should combine for the speedy achievement of the goal before us, the building of a Communist society.

Our science cannot tolerate indifference to our ideals, it cannot tolerate scholasticism and metaphysics.

In agriculture, which deals with vegetable and animal organisms, special importance attaches to biology as a science which investigates the laws of development of living organisms.

Biological science can render practical workers invaluable help, provided, however, it proceeds from materialist dialectics and creatively develops the theoretical legacy of such giants of our Soviet biological science as Timiryazev and Michurin.

However, in biology, not all scientists are as yet following the path mapped out by the best Russian biologists. Along with the progressive, materialist Michurin trend, there is another trend, diametrically opposed to it, an anti-Michurin, essentially reactionary and idealist trend in science.

The Michurin trend in biology proceeds from materialist positions. It maintains that changes in the conditions of life inevitably change the nature and forms of organisms. The nature of organisms is built up by the conditions of life and is changed by the conditions of life.

The Michurin trend maintains that new properties of organisms, acquired in the course of their development under the influence of changed conditions of the environment are transmitted to the offspring, are inheritable.

The Michurin teaching arms the practical workers with scientific methods of changing and perfecting the nature of plants and animals in a direction desirable to man.

The most consistent continuer of the advanced, progressive Michurin trend in biology is Academician Lysenko. He is rendering great service by waging a courageous, principled fight against the idealist dogmas of Mendel-Morgan genetics (Weismannism), against metaphysical and formalistic propositions in biology, and by contributing to the rapid development of the Michurin teaching, which represents a new stage in materialist biology.

And it is not a mere accident that advocates of the Mendel-Morgan trend prefer not to mention Michurin, minimize the great theoretical and practical significance of his works, and at the same time widely advertise what they claim are scientific achievements of Mendelism-Morganism. That, for example, is the position held by Academician Schmalhausen to this day. How else are we to explain the fact that in his book *Factors of Evolution,* published in 1946, no mention is made of such giants of scientific thought as Michurin and Timiryazev who devoted their entire glorious life of creative activity to these questions?

This attitude of Academician Schmalhausen to the works of Timiryazev and Michurin is not an accident. It is undoubtedly due to his anti-Darwinist, anti-Michurinist views, although he and his defenders represent them as Darwinism.

For the laws of living nature Academician Schmalhausen substitutes formalistic schemes which lead him to anti-scientific conclusions about allegedly inevitable blind alleys of evolution.

That is the logical end arrived at by investigators who are divorced from life and from advanced science.

It must be obvious to everyone that the views of Academician Schmalhausen and his adherents are detrimental to our agriculture, because they poison the minds of practical agriculturists and students of our agricultural schools with idealism and metaphysics, they handicap agronomists, animal breeders and other farming experts.

From this pseudoscientific position springs the attitude of unfriendliness towards the Michurinists and of simply ignoring their remarkable achievements, first and foremost those of Academician Lysenko.

The formal geneticists, as their speeches at this session have shown, are bent on defending reactionary theories.

They are trying hard to this day to make it seem that Lysenko has nothing in common with Michurin.

Such attempts to oppose Michurin's teaching to the views and beliefs of Academician Lysenko, who is developing and adding to this teaching, are unseemly and we must condemn them. (*Applause.*)

Attempts to reconcile the two different trends in biological science, to combine the reactionary trend in biology with the

progressive Michurin trend, or to follow some middle line, as Academician Zavadovsky proposes, are inevitably doomed to failure.

Academician Lysenko, while engaged in creative effort, developing and advancing the theory of biology, maintains close contact with collective farms and state farms. We in the Ministry know more than anyone else what great help Academician Lysenko renders the practical workers (*applause*), how he organizes their forces for the speedy application of the achievements of advanced science. His activity is an example for every Soviet scientist to follow; for only in close contact with practice can advanced science develop successfully. (*Applause.*)

The anti-Michurinist trend in biology proceeds from metaphysical conceptions and denies the possibility of qualitative changes in the hereditary properties of plants and animals under the influence of changing conditions of life. It denies that it is possible for living organisms to transmit properties and qualities which are produced in the living body under the influence of changing conditions of life. The anti-Michurinist trend proceeds from the false assumption that man cannot know the causes which give rise to and direct changes in heredity.

The profoundly reactionary and utterly mystical view handicaps practical breeders in their work to produce new varieties of plants and breeds of animals. It reduced the role of the breeder to that of a treasure hunter, to the role of a man who passively waits for the appearance of desirable forms of plants and animals.

The defenders of this trend are captives of the bourgeois theories of Morgan, Mendel, Weismann. They worship bourgeois science to this day, and wittingly or unwittingly contaminate science with anti-scientific ideas.

The adherents of the anti-Michurin trend in biology, often citing Darwin as their authority, forget that the Darwinian theory only explains the evolution of the organic world, whereas Michurin and his followers teach how to change forms according to plan, how to create new forms, how to transform the nature of plants and animals in a direction useful to man.

The biologists who follow the anti-Michurinist path are detached from life and practical activity. Their barren investigations can contribute nothing to help the collective farms and state farms.

We do not know of any results valuable from the practical standpoint which have been obtained by the formal geneticists on the basis of Morganism-Mendelism. And a science which does nothing to help in production, which supplies no weapons to practical workers, which does not aid Soviet people in their efforts to build up an ever better life is not a science which we can call ours. (*Applause.*)

Academician Lysenko and others who have spoken in the discussion at this session have quite rightly and deservedly levelled sharp criticism against the anti-Michurinist trend in biological science.

The speeches of the formal geneticists have shown that they stick to their old reactionary positions and apparently intend to continue to serve as an impediment to the development of advanced biological science.

That is the unenviable role assumed by the group of formal geneticists who have abandoned the principles of materialist dialectics.

The representatives of formal genetics have not realized the unsoundness of their theory, have not found the courage to admit that their views are erroneous, have drawn no conclusions for themselves from the speeches of numerous scientists and practical workers, representatives of Ministries, at this session —speeches reflecting not just the personal views of the speakers, but the views of large numbers of scientific workers, agricultural experts and collective farmers. (*Applause.*)

In his speech yesterday, Academician Zhukovsky clearly defined his attitude to Mendelism. He even called upon Soviet biologists to bow before Mendel and vow fidelity to his doctrine.

Our answer, Academician Zhukovsky, to your call to unite on the basis of Mendelism is: If you persist in treading the old path of Mendelism, our ways must part. (*Applause.*)

We Michurinists reject the reactionary, idealist biology, and together with the collective farmers, together with our Lysenko, we will fight under the banner of the advanced, progressive

Michurin teaching for the further flowering of materialist biology and for ever more scientific assistance to the state farms and collective farms. (*Applause.*)

In the light of the great tasks facing the Academy, the questions of scientific propaganda for the further development of the Michurin teaching assume particular significance.

It will be necessary to revise as speedily as possible the work plans of scientific research institutions.

It will be necessary to expand in every way the research work with a view to elaborating the immediate problems posed by the Michurin teaching in close connection with the measures to solve the major questions of farming.

It will be necessary to effect a radical change in the system of teaching breeding and genetics in agricultural colleges and high schools, to make sure that these subjects are taught on the basis of the Michurin doctrine.

This inevitably means that the respective departments in colleges and other schools and the sections of genetics and breeding in scientific research institutes will have to be reinforced with scientific workers who have proved in practice their ability to apply their creative effort to the development and propagation of the progressive ideas of the Michurin doctrine.

For the correct education and formation of forces of biologists, plant and animal breeders, agrotechnicians, zootechnicians and other farming experts it is extremely important to have textbooks equal to the level of contemporary knowledge and the lofty ideas of Michurin, Williams, Lysenko and other advanced Soviet scientists.

Soviet scientists, boundlessly devoted to their people, and equipped with the theory of Marx-Engels-Lenin-Stalin, the most advanced theory in the world, will ensure the further development of the progressive teachings of Michurin, Williams and Lysenko and their translation into reality.

There is no doubt that our scientists, enjoying as they do the constant solicitude of the Party and the Government, will soon achieve new remarkable successes which will contribute to the further powerful development of our socialist agriculture and thereby justify the high confidence placed in them by the Soviet people and its great leader, Joseph Vissarionovich Stalin. (*Loud applause.*)

Academician P. P. Lobanov. I call upon Academician V. S. Nemchinov.

Academician V. S. Nemchinov. Comrades, not being a biologist by education, I did not intend to speak at this session. All the more that such leading workers of the Timiryazev Agricultural Academy have taken the floor here as I. V. Yakushkin, V. P. Bushinsky, D. A. Kislovsky, P. M. Zhukovsky, and A. R. Zhebrak. In other words, practically every major department of the Timiryazev Academy which has any relation to agronomical knowledge and agricultural science has been represented.

I observe that there is no unity among our scientists on certain questions, and I personally, as director of the Timiryazev Academy, see nothing bad in this. (*Commotion.*)

In our Academy we have sixty departments of different trends and specialities. On the basic questions of agricultural science and practice there is complete unity and harmony among us, and I consider there is nothing bad in the fact that there are certain divergencies, or, rather, different points of view, among us, since these divergencies, in my opinion, are not fundamental. (*Commotion.*)

I have been directing the work of the Timiryazev Agricultural Academy for eight years. And I felt obliged to take the floor only because Comrade Simonov and Comrade Demidov said that it is necessary to throw light on the work of the Timiryazev Academy.

Comrade Simonov asserted here that there was a tendency in the Academy to get rid of all who do not conform, that this applied in particular to the Michurinists, and among them he mentioned a number of comrades: himself, Alisov, Tikhonenko and some others.

I deem it necessary to inform this session that Comrade Simonov left the Timiryazev Academy because he was appointed director of the Michurin Agricultural Institute.

A voice. He did not speak of himself.

I. N. Simonov. I did not refer to myself.

V. S. Nemchinov. He told me personally that his appointment as director of the Michurin Agricultural Institute was a measure which was to be regarded as aggressive. It is another

matter that Comrade Simonov has not made a success of his job, but that is his own fault. At any rate, he is wrong. He is equally wrong with regard to Comrade Tikhonenko, with whom I have spoken. If they would release him from his present work, he would have returned to the Academy long ago.

There is no established regulation that all who graduate from the Timiryazev Academy are obliged to stay and work there. On the other hand, I would have accepted the reproach that the Academy does not send enough of its people to the provinces to reinforce the agricultural colleges. That would have been a correct reproach. That would have touched a weak spot that really exists with us, for we have the greatest difficulty in inducing people who have grown up and received scientific qualifications within the walls of the Timiryazev Academy to go and work in the provinces.

Comrade Simonov said that Professor Rubin was not selected for a chair in the Timiryazev Agricultural Academy solely because he was a Michurinist. That is not true, it is a calumny, and Comrade Rubin can confirm that the Timiryazev Academy committed a mistake in regard to him in choosing another candidate. I told him this personally, and Professor Rubin can corroborate it. But mistakes occur in every organization.

I have never heard from Comrade Simonov that he was compelled to leave the Timiryazev Academy. Why does he say such things? It absolutely does not conform to fact.

A voice. You only produce scientists like Lomako.

V. S. Nemchinov. I do not know why you say we produce scientists like Lomako. Lomako, after all, is a worker of the Byelorussian Academy of Sciences, and not of the Timiryazev Academy. So that does not conform to fact.

Nor do other assertions made here conform to fact. Unfortunately, I did not hear Comrade Simonov's speech, but when I got the verbatim report I read it very carefully.

Comrade Simonov cited as an illustration of our anti-Michurin attitude that we, as he alleges, rooted out our currant bushes. This absolutely does not conform with fact. We may have pulled up a few bushes, but our work on gooseberries and currants is not to be judged by that. I invite you all as a body to drive out to Otradnoye, and we shall show you a veritable wealth of gooseberry varieties.

Comrade Simonov made free to talk here of meek and obedient professors. I consider that such an assertion does no credit to the man who makes it, that this sally of Comrade Simonov's was absolutely impermissible.

It is said that there is no Michurinian trend in the Timiryazev Agricultural Academy, that our workers, professors and so on are anti-Michurinists. That is not true.

A voice. No one said this of all the professors.

V. S. Nemchinov. Will anyone say of N. N. Timofeyev, head of the department of fruit and berry breeding, that he is an anti-Michurinist? And who can say this of Comrade Kolesnichenko?

However, there are scientists among us of a different trend, notably Professor Zhebrak. Comrade Simonov declared that Nemchinov, the director, approved Professor Zhebrak's article published in a foreign journal. This comes close to downright calumny, for it does not conform with fact. It should be said that the public knows that one of the first to come out in the press against A. R. Zhebrak's article was none other than Nemchinov.

A voice. The first were the writers.

V. S. Nemchinov. I said I was one of the first, at any rate, immediately after the writers' article.

A voice. Tell us about your letter to the *Leningradskaya Pravda*.

V. S. Nemchinov. I never wrote any letter to the *Leningradskaya Pravda*, and I don't know what you are talking about. This is obviously some myth.

In my speeches, in my reports at Party meetings and in the Council of the Academy, I dissociated myself from Comrade Zhebrak's article and assessed it as it deserved. All the comrades that have spoken here on this subject know this very well, but for some reason they deem it necessary to lead the Soviet public astray.

A voice. They sense the truth.

V. S. Nemchinov. The truth, of course, will aways remain the truth, and it will triumph in the end.

As the director, I may be reproached with one thing, and that is that I make a distinction between the article of Professor Zhebrak, Member of the Byelorussian Academy of Sciences,

and his work. I said that Professor Zhebrak's article to the American journal was condemned not because he adheres to and upholds the chromosome theory of heredity, but because he had committed an unpatriotic act. And so it was.

A voice. Is the chromosome theory part of the gold fund?

V. S. Nemchinov. Yes, I may repeat it, I do consider that the chromosome theory of heredity has become part of the gold fund of human knowledge, and I continue to hold that view.

A voice. But you are not a biologist, how can you judge?

V. S. Nemchinov. I am not a biologist, but I am in a position to verify this theory from the point of view of the science in which I am doing research, notably, statistics. (*Commotion.*)

And it also conforms with my ideas. But that is not the point. (*Commotion.*)

A voice. How is it not the point?

V. S. Nemchinov. Very well, let it be the point. But I must then declare that I do not share the view of the comrades who assert that chromosomes have nothing to do with the mechanisms of heredity. (*Commotion.*)

A voice. There are no mechanisms.

V. S. Nemchinov. You think there are no mechanisms. But this mechanism can not only be seen, it can be stained and defined. (*Commotion.*)

A voice. Yes, stains. And statistics.

V. S. Nemchinov. Nor do I share the view which was expressed here by our respected chairman that the chromosome theory of heredity and, in particular, certain Mendelian laws, are an idealist standpoint, a reactionary theory. I personally consider this opinion wrong, and this is my point of view, although it may not be of much interest. (*Commotion and laughter.*)

A voice. It is of great interest.

V. S. Nemchinov. I have never concealed this point of view. It is my point of view, the point of view of a man who is not a specialist.

A voice. Coming from the director of the Timiryazev Academy, it would be interesting to hear it.

V. S. Nemchinov. Well, then, allow me to give it. I would not consider it right if A. R. Zhebrak, who committed an anti-

patriotic action, which was condemned as it deserved—I do not consider that it is necessary because of this to close down his work on amphidiploidy.

A voice. You ought to resign.

V. S. Nemchinov. Perhaps I ought to resign. I do not cling to my post. (*Commotion.*)

A voice. And that's bad.

V. S. Nemchinov. But I consider my view correct, and the aggressive character of the statements and actions aimed at prohibiting the work of A. R. Zhebrak I consider incorrect. It was not for nothing that the Ministry of Agriculture has sanctioned the establishment of a genetics base at the Academy.

A voice. And what is the base doing?

V. S. Nemchinov. I don't know whether you have been at the base or not. I have, and have acquainted myself with its work, and in my opinion in the very near future, the results of that work will be apparent. The fact that they are not apparent to us yet....

A voice. How much time will it require?

V. S. Nemchinov. A. R. Zhebrak has said that results will be forthcoming in the very near future.

But that is not the point. I cannot share the view that A. R. Zhebrak and members of the staff who adhere to the chromosome theory should not work at the Timiryazev Academy. I believe they ought to continue to work there, just as, similarly, I do not consider that adherents of the Michurinian view should be driven out of there.

A voice. You have driven them out, there is no one left to drive out.

V. S. Nemchinov. We have N. N. Timofeyev, I. V. Yakushkin, V. P. Bushinsky, Reznichenko and others who hold to the Michurinian trend working with us. Many of our scientific workers, including A. R. Zhebrak, are working actively on the basic problem—the remoulding of nature in the interest of socialist agriculture.

A voice. Tell us what is your attitude to the principles of the address.

V. S. Nemchinov. My attitude to T. D. Lysenko's address is as follows. His basic precepts and basic ideas, the purport of which is to mobilize agrobiological science in the service of

collective farming and to transfer the methods of his work to all the collective-farm fields, I consider correct.

A voice. Theoretically.

V. S. Nemchinov. As to the theoretical side of it, I consider that in the matter of the chromosome theory of heredity Trofim Denisovich Lysenko is wrong.

A voice. But you're not a specialist.

V. S. Nemchinov. No, I am not a specialist, that is why I did not speak until you asked me.

I am watching T. D. Lysenko's work with great attention and interest and I have immense respect for his ability to direct agronomical science in the interest of collective farming, but that does not mean that I fully agree with him. I agree with him on a number of things, but on certain questions I cannot agree with him.

A voice. On the fundamental thing.

V. S. Nemchinov. No, I do not regard this as fundamental, I cannot agree with that and I consider it wrong.

A voice. And other fundamentals?

V. S. Nemchinov. Other fundamental views I consider correct. (*Commotion.*)

But that is a very important difference, and since I am not a biologist, it must be settled by biological scientists.

The reproach is levelled at me that the workers of the Timiryazev Academy have done nothing for agriculture.

I. I. Prezent. That does not apply to all of them.

V. S. Nemchinov. Take our plant breeders. After P. I. Lisitsyn, the department is now headed by P. N. Konstantinov. Can you say that he has done little for agriculture? His varieties are cultivated on millions of hectares.

S. S. Perov. What have the genes to do with it?

V. S. Nemchinov. Genes? It is you that keep bringing them up. (*Laughter.*)

I say that the cereal breeding department is headed by P. N. Konstantinov, and you talk about genes. You say that breeding and seed growing are in the hands of anti-Michurinists. The department of fruit and vegetable breeding and seed husbandry is headed by N. N. Timofeyev. He is a real and genuine Michurinist. What are you then accusing me of? You say—"genetics." But genetics is only a small course,

delivered only in the department of plant breeding and seed husbandry.

I. N. Simonov. It is the spoonful of tar which spoils the barrel of honey.

V. S. Nemchinov. If you consider A. R. Zhebrak's work on experimental genetics a spoonful of tar, I am of a different opinion. A. R. Zhebrak is a scientist of merit, and I consider that the thought of removing him from the Timiryazev Academy cannot be entertained. He grew up in the Academy.

I. I. Prezent. Your product, your creation!

V. S. Nemchinov. There are products and products.

It seems to me that all the charges levelled at me of belonging to the anti-Michurinian trend fall to the ground. Names like those of N. N. Timofeyev and P. N. Konstantinov speak for themselves. There remains only A. R. Zhebrak. But as to A. R. Zhebrak, I have given my answer as director.

F. A. Dvoryankin. We are talking about the line, not about that.

V. S. Nemchinov. As to the line, it is to promote Williams' teachings in the Timiryazev Academy to the utmost. We have followers of the Williams trend. They are direct heirs and successors to V. R. Williams' work—Professor Bushinsky, Professor Chizhevsky and a number of other workers. We also have a special soil station named after Academician V. R. Williams.

F. A. Dvoryankin. You are turning Timiryazevka into a Nemchinovka.

V. S. Nemchinov. If that were so, I should not have been the director for eight years. I do not agree with Academician T. D. Lysenko on a number of material questions. In particular, I do not agree with him when he categorically declares that the Institute of Agricultural Economy should not be attached to the Academy of Agricultural Sciences of the U.S.S.R., but should be under the Ministry of Agriculture.

I. I. Prezent. That is not a question of principle.

S. S. Perov. No principle is involved here.

V. S. Nemchinov. It is a question of principle, because to consider that the Agricultural Academy is limited solely to agrobiology is wrong.

I. I. Prezent. Nobody says so.

V. S. Nemchinov. I continue to hold to this and would be glad if this session were to instruct the Presidium of the Academy to found an Institute of Economy, for otherwise what was the point of co-opting four academicians on agricultural economy to the Academy.

I bear the moral and political responsibility for the line of the Timiryazev Academy. I am morally and politically responsible for the line I pursue, and I consider it right, and will continue to pursue it. If this line should prove to be wrong I shall be corrected, or the hopes and desires which are expressed here will be fulfilled, so that I resign my post. It is impermissible, in my opinion, to shut down at the Timiryazev Academy the work of Professor Zhebrak.

I. I. Prezent. Of the Morgan type, not so?

V. S. Nemchinov. I bear the moral and political responsibility, and shall continue to bear it as long as I am entrusted with it.

A question. Do you know that Professor Paramonov in his lectures on Darwinism misrepresents and vilifies the work of Lysenko and his whole school?

V. S. Nemchinov. I am familiar with Paramonov's lectures. He does not vilify or misrepresent Darwinism, and if on certain questions he does not agree with T. D. Lysenko, this cannot be regarded as vilification. Professor Paramonov is a first-class lecturer, a favourite with the students, a Darwinist to the tips of his fingers, a Timiryazevian from beginning to end. If he does not agree with certain views—that is another matter. That is not vilification of Lysenko. I have conversed with Paramonov many times, I attend his lectures, I read the stenographic reports of his lectures.

A voice. We have also heard his lectures and know that it is true.

V. S. Nemchinov. Then let us have the documents.

A voice. What documents?

A question. Do you know that in the lectures on Darwinism at the Timiryazev Academy the names of Timiryazev and Michurin are not mentioned even rarely?

V. S. Nemchinov. Utter nonsense! (*Laughter.*) I have many times attended the lectures of Professor Paramonov and know his standpoint.

A question. Tell us, please, do you think that the substance

of the course on genetics should be Michurinian? I earnestly request you to answer this question.

V. S. Nemchinov. I consider that the course on genetics should reflect the views of Academician T. D. Lysenko on phasic development and a number of other of his works. The principles of the chromosome theory of inheritance should likewise not be kept from the students. (*Commotion.*)

A question. Tell us how you intended to dismiss Professor Kislovsky?

V. S. Nemchinov. I know nothing about it. Professor Kislovsky is present here, and I think he will corroborate that there was never an instance when I did not support him in his work. My attitude to Professor Kislovsky has always been the best. If this is not so, let him say so here.

A question. If you can, tell us what varieties have been produced at the Timiryazev Academy by Michurin's methods. Give us the data regarding the variety trials.

V. S. Nemchinov. I might mention, for instance, the Timiryazev variety of gooseberry, which was produced by Michurin's methods and which we are multiplying.

Academician P. P. Lobanov. I call upon the Assistant Director of the Institute of Genetics of the Academy of Sciences of the U.S.S.R., Comrade V. N. Stoletov.

V. N. Stoletov. Academician V. S. Nemchinov has just had the floor. Permit me therefore to say a few words about the Timiryazev Academy.

Some ten years ago, one of our solid agricultural journals printed an article criticizing fundamental errors in the methods of a certain worthy academician, a professor of the Timiryazev Academy. The academician read the article and was indignant. He came to the editor and with an affected air of nonchalance said: "The author of the article is illiterate; he should be going to school, not criticizing me." He then went on to say: "The journal's mistake can be corrected. Call your people together, and I shall give them two or three lectures. Then everything will be clear, and the editors will admit their mistake." He seriously thought, apparently, that a fundamental difference could be stifled by a lecture.

I was reminded of this incident by some of the speakers from the Timiryazev Academy who spoke here yesterday.

A voice. And supplemented here today!

V. N. Stoletov. Professors A. R. Zhebrak and P. M. Zhukovsky of the Timiryazev Agricultural Academy at first had no desire, it seemed, to speak at this session. They apparently intended to keep silent and let the thing blow over, so to speak. When this did not succeed, they got up and began to deliver "educational" lectures from the platform of this session. They pretended not to understand what the whole thing was about. In this respect, Academician V. S. Nemchinov's speech was very illuminating. It is now perfectly obvious that Academician Nemchinov fully shares their point of view, and at the same time, evidently, approves their conduct.

It has been remarked in the course of the debate that the heads of the Timiryazev Academy do not want to have workers in their institution who guide themselves by the Michurinian doctrine.

Academician V. S. Nemchinov either denied the charges levelled against him under this head, or classed the facts communicated as having been due to mistakes. A director, of course, may make mistakes. But there are mistakes and mistakes. There are mistakes which are deliberate and premeditated. It seems to me that it is just because of such deliberate "mistakes" on the part of the heads of the Timiryazev Academy that the Michurinists there feel so uncomfortable.

Academician Nemchinov gave us here his personal opinion on the subject of the chromosome theory of heredity. A personal opinion must, naturally, influence one's work in one way or another. And that influence is to be observed.

Timiryazevians, past and present, have a warm love for their "Timiryazevka." But they love the "Timiryazevka" of V. R. Williams, the "Timiryazevka" of M. F. Ivanov, and have no respect for the "Timiryazevka" of A. R. Zhebrak, who propagates the reactionary ideas of Morganism. (*Applause.*) The majority of the Timiryazevians must have been grieved to hear Academician Nemchinov's declaration that in his personal opinion the chromosome theory has gone into the gold fund of science.

V. S. Nemchinov. It certainly has. That is my opinion.

V. N. Stoletov. In our view this "contribution" to science does nothing but harm to real, materialist biology.

A voice. Hear, hear!

V. N. Stoletov. In his very first statements against Mendelism-Morganism (1935-1936), Academician Lysenko warned his hearers and readers that what was involved was not minor questions of biology, but the principles, the direction, of biological research. The fundamental divergence among the biologists springs from different views concerning the process of evolution of the vegetable and animal world. The character of a biologist's researches, and the success of his researches, to a large degree depend on whether he holds to the scientific, materialist position, or whether he is a captive of idealism, of metaphysics.

We have to know the true laws of development of plant and animal forms in order to be able in practice to direct the creation of new forms, the creation of desirable forms of plants and animals. Obviously, proper knowledge of living organisms depends primarily on scientific methods of thought. To T. D. Lysenko, the basic aim of the discussion was to uphold scientific methods of thought in the study of biological laws.

Furthermore, to him it was always an incontrovertible axiom that scientific thought in biology must be upheld, not in scholastic disputes but by persistently studying objective reality, by constantly verifying the knowledge gained of the laws of nature, by testing it in the crucible of practice, of experiment. Causality, necessity, must be sought for in objective reality; and it is in objective reality that the laws found and discovered must be immediately tested, by endeavouring ever more effectively to govern living nature practically in the interest of man.

The guiding ideas for the solving of so important a biological question as the causes of variability in organisms, T. D. Lysenko sought for and found in the classics of Marxism-Leninism. He said in one of his lectures that we may find in the works of our teachers of Marxism-Leninism, not only general guiding ideas for the study of heredity variability, but also direct and concrete indications as to what variations are due to, the ways in which they arise in organisms.

T. D. Lysenko did not reply to his opponents by referring to general guiding ideas, or by quotations. His reply was the success of his researches, his experiments, which are so com-

prehensible and graspable, and whose theoretical principles are so profound. To T. D. Lysenko, the centre of the discussion was not in conference halls, but on the fields of the collective farms and state farms, in hothouses, and on experimental plots. He demonstrated the scientific soundness of his biological thought by increasing the yield of self-pollinators after intra-varietal crossing, by increasing the yield of cross-pollinating plants as the result of open cross-pollination, by increasing the yield of the potato in the South, by improving its breed qualities under the influence of summer planting, by obtaining high yields of millet, by creating scores of vegetative hybrids, and by deliberately converting any winter form into a hereditary spring form and, vice versa, spring forms into winter forms.

For T. D. Lysenko, the years of discussion were filled with intense research work into cardinal problems of genetics. But in these same years he also continued, guided by the theories elaborated, to solve major economic problems. With him, the solving of economic problems did not go parallel with fundamental research; it was intertwined with it, and arose out of it.

This was not the way of the Morganists. It did not occur to them to make experimental studies of Lysenko's principles. On the contrary, they mustered every genetic hypothesis, assembled all the old and misunderstood facts, and with all this rather outmoded arsenal attacked T. D. Lysenko's positions. To all the new and verified facts persistently brought forward by T. D. Lysenko, the Morganist-Mendelists invariably opposed these old hypotheses, together with a few facts.

Evidently with the idea of maintaining his "style," Professor A. R. Zhebrak yesterday exhibited from this platform sheaves of wheat which we have been seeing for about three years.

A voice. More.

V. N. Stoletov. These sheaves were from the 1945 harvest. Is it that the experimenter has grown nothing new in this period? Apparently, not.

All through the discussion, it mattered very little to our native Mendelist-Morganists that the country was expecting of them some real contribution to biological science. The Mendelist-Morganists began attacking Academician Lysenko solely for the purpose of defending their Weismannistic ideology.

In the course of the discussion Academician Lysenko and his followers shattered the theoretical foundations of Weismannism in our biology. All thinking persons were convinced that Weismannism is alien to our world outlook, and that in practical work our Weismannists, like the king in the fable, are naked.

The paths of the Michurinists and Morganists have diverged into two diametrically opposite directions.

If the situation in biology were such that the differences among our biologists concerned minor scientific questions, the matter could have been settled quite simply. Professor A. R. Zhebrak, Academician I. I. Schmalhausen, Professor N. P. Dubinin and the other Morganist-Mendelists could have been gathered together and taken for an excursion around our research institutes where Michurinists are working, our plant-breeding stations, and the scientific truth and potency of the Michurinian theory could have been demonstrated to them by numerous experiments. They could have been shown real vegetative hybrids, which help us to understand the nature of heredity and its variability in the right way, the Michurinian way. Today we can already show that there is not a single grafting, if it is done properly, of course, which does not produce variations. They could have been shown scores of spring forms obtained from winter forms, and winter forms obtained from spring forms.

On the way from one institute to another, they could have driven to some of our experimental fields in the southern Ukraine and examined the results of tests of varieties obtained by means of directed alteration. These new varieties are in quality superior to the old. Further, they might have been convinced by the experiments of the Michurinists that sexual hybrids do not segregate in the Mendelian way. Nowadays, the inapplicability of the Mendelian laws can be demonstrated also on the *Drosophila*.

We now have experiments demonstrating one of the fundamental principles of the Michurinian genetics—the genetic diversity of the cells and tissues of plants. Experiments may be shown proving the inheritance of acquired characters (and in the case of the *Drosophila* as well). The Michurinists have experiments which convincingly prove that hybridization is not a shuffling of permanent genes, as the Mendelist-Morganists think,

but a means of obtaining "destabilized" organisms, from which, by means of training, new forms of plants may be created, possessing properties which neither of the parents possessed.

The Michurinists have abundant experimental data which expose the scientific unsoundness of the chromosome theory of heredity. But our Morganists do not wish to understand, to scientifically analyze, the facts obtained by the Michurinists. They often declare: prove to us that the chromosome theory is wrong, and we shall agree with you, Michurinists. But they do not want to be convinced, they do not want to re-examine the foundations of their unscientific theories, which rest on Weismannism. To convince under such circumstances is difficult, impossible.

Our Morganist-Mendelists often cite tetraploid kok-saghyz as evidence of practical achievements obtained on the basis of the chromosome theory. But in our opinion this fact has nothing to do with the chromosome theory. Tetraploid kok-saghyz was obtained by the action of external factors on the organism, whereas the crux of the chromosome theory is that the development of organisms and their variability are determined by unknowable forces latent in the chromosomes. The plan of the architect and the forces which carry out the plan lie in the chromosomes, says Schrödinger (whose book was criticized as idealistic by the President in his address).

Yet our Morganists often cite tetraploid kok-saghyz, in spite of their own theory.

Why do they do so? I must state at this point that Academician T. D. Lysenko is not one of those who discard anything that is practically useful. On the contrary, he always welcomes everything useful. And he has studied tetraploid kok-saghyz far more thoroughly than the Morganists. Why the Morganists need this kok-saghyz is to back their unscientific position in biology. They make so much noise about it for one sole purpose, and that is to legitimatize Mendelism-Morganism in our biology.

One well-known Morganist, M. S. Navashin, the man who was most closely connected with the development of tetraploid kok-saghyz, once inadvertently admitted that it was made such a point of, not because there were facts pointing to the practical value of this variety—there are many valuable plants in our country—but that it was so much advertised because the "achievements" of Mendelism-Morganism were finding no sup-

port, because Mendelism-Morganism was being opposed by the Michurinists.

The Mendelist-Morganists endeavoured, with the help of tetraploid kok-saghyz, to conceal the reactionary essence and barrenness of Mendelism-Morganism.

As a rule, when a Soviet scientist succeeds in obtaining a new form of some cultivated plant, he is obsessed with one concern, and that is to multiply it as quickly as possible, to test it as thoroughly as possible, to study it, and then—if it proves good and useful—to introduce it into practical farming on a large scale.

But that is not the way of the Mendelist-Morganists. When they every now and again bring up the question of tetraploid kok-saghyz and try to get certain measures taken about it, it is not practical measures for its better study and multiplication they are out for (in this respect all the facilities are available; Navashin himself admits that the intervention of high-placed authorities is not needed). It is not practical measures the Mendelist-Morganists are interested in, but in legitimatizing in our biology, in opposition to the Michurinian trend, a second trend, the reactionary Mendelian-Morganian trend.

The Morganists were infuriated by the fact that our agricultural authorities confined themselves in the case of tetraploid kok-saghyz to purely practical measures, and declined to popularize Mendelism-Morganism.

The Morganist-Mendelists were infuriated by the fact that they failed, under cover of tetraploid kok-saghyz, to legitimatize Mendelism-Morganism in our biology.

We may rest assured that tetraploid kok-saghyz will be thoroughly studied by our research institutions. If it proves to be a useful form, it will be introduced into practical farming. But then, we think, the Mendelist-Morganists will lose all interest in it.

No devices of the Mendelists can save the chromosome theory from complete exposure as barren and unscientific.

Of late years, the Mendelist-Morganists have frequently been resorting to abominable methods of scientific struggle in defence of their ideas: they pose as Michurinists, they say they believe in Michurin, and if they fight at all, it is only against Lysenko.

Academician B. M. Zavadovsky went even further. He said that he was perhaps the first to take up the fight against Morganism-Mendelism.

We remember very well what sort of a fight it was that B. M. Zavadovsky waged against Mendelism-Morganism. It was more a lover's quarrel than a fight. The best evidence of this is his speech at this session. While declaring that he was fighting Mendelism-Morganism, B. M. Zavadovsky actually upheld the views of such "opponents" of Morganism as those one hundred per cent Morganists, Professor Dubinin and his closest associate, Romashov.

This is best of all revealed by the attitude of the Morganists to Michurin.

In 1940, the Morganist, Romashov, wrote: "I shall expound Michurin from the viewpoint of a representative of genetics who in Michurin's work has familiarized himself with the new chapters in genetic science. It is my firm conviction that these chapters do not in any way contradict the fundamental principles of the classic genetics [alias, Morganism]. This conclusion was one of the results of my study of Michurin's works."

A voice. This Morganist read "productively"!

V. N. Stoletov. Romashov went on to say, in distortion of the facts, that in none of Michurin's works were there any data which "contradict the fundamental principles of modern genetics and the chromosome theory of heredity."

The differences between Michurin and the Mendelist-Morganists, according to Romashov, relate solely to "the specific objects with which Michurin worked." There is no need to say that this is a downright falsification of Michurin.

Academician T. D. Lysenko has shown that the laws established by Michurin in the case of fruit trees are real laws, and are applicable to the whole vegetable world. He disclosed the general significance of Michurin's theory for biology. No doubt it is for this reason that the Morganists are so determined to drive a wedge between Michurin and Lysenko.

Romashov, guided by Dubinin, misrepresents Michurin. But, at least, he has never said anywhere that he is an opponent of Morganism. He is a one hundred per cent Morganist. And B. M. Zavadovsky spoke of the attitude of Michurin to Mendelism essentially in the same style, only asserting that he, Zava-

dovsky, is an opponent of Mendelism, that he has fought the Morganists.

B. M. Zavadovsky said that Michurin should be read in the original, obviously hinting that Lysenko misrepresents Michurin.

It should be said that there are two ways of reading any work—in this particular case Michurin's works. One may be called the Lysenko way; the other, the Zavadovsky, or Morganist, way. The Lysenko way is to read Michurin diligently and constantly and to find means of solving actual problems of contemporary theory and practice—to read Michurin in order constantly to develop and perfect him. That is the creative way of studying Michurin.

Zavadovsky's way, to put it mildly, is a scholastic way. It is evident that he finds it necessary to read the works of Michurin solely in order to find in them confirmation of his long-established, already petrified, ingrained formal ideas. In his way of reading Michurin, B. M. Zavadovsky reminds us of the Chinese emperor who after every lesson in mathematics thanked his teacher for reminding him of forgotten truths, which he, the emperor, could not but know, seeing that he was the omniscient Son of Heaven. B. M. Zavadovsky pores over the works of Michurin, not in order to cull from them guiding ideas for theoretical and practical work, but in order to find confirmation of his own, already firmly established convictions.

In short, science to the live Lysenko is a living thing, but to the formalists (and you may class in this general category any of the Morganists that spoke here), science is a formal thing.

Dr. Rapoport, who spoke in the debate, tried to assure his audience that sometime in the future the Morganists will bless mankind with great discoveries. Be it remarked that this is not only his idea—it is the idea of all our native Morganists. They all try to show that Mendelism has given much of practical value in the past, and will give still more in the future. Our present-day Morganists are doing their level best to take credit for the varieties produced by Lisitsyn, Shekhurdin, Yuryev and other well-known plant breeders. Let us grant for a moment that Mendelism in the past did have some hand in the creation of varieties which are now widely distributed. But the question at once arises: why has Mendelism-Morganism become impo-

tent today, why don't plant breeders use it today in the production of new varieties? It appears that Mendelism-Morganism was fruitful in the past, and will be fruitful in the future, but is impotent today. It appears that Mendelism has so far not produced a new method, begs us to wait, and meanwhile the old methods have become antiquated and for that reason are not being utilized. Usually, this "condition" is not characteristic either of life, or of truth, or of science. Every method becomes antiquated when a new and better one appears. Summer planting in the South, for example, will always improve the breed qualities of potato.

In actual fact, the matter with regard to Mendelism is much simpler. From the point of view of practical usefulness, Mendelism was hollow in the past, is hollow today, and will evidently be hollow in the future.

As regards the varieties which the Mendelist-Morganists are now trying to appropriate, the way they arose was correctly described by Professor S. I. Zhegalov some time ago. He wrote that these varieties could only have been obtained by selection, and selection is practised only on the basis of the firmly established fact that among all self-pollinators, including wheat, a large number of minor forms exists. The method of analytical selection by which these varieties were obtained, Zhegalov concludes, lends meaning to an aphorism credited to Jordan: "In order to produce a new variety, we must first have it." Our best-bred varieties are the result of selection from local peasant varieties. Our country pays due honour to the breeders who made the selections. But Mendelism-Morganism played no part in their work. It may only be said that, thanks to Mendelism-Morganism, there was a time when our local varieties were neglected. It was only the interference of the Party and Government that prevented the complete destruction of the local varieties.

Having nothing with which to counter the Michurinian criticism in substance, the defenders of Mendelism-Morganism have in recent years often said that Lysenko is gagging them, that it is impossible to discuss with Lysenko. An exhaustive reply to this has been given in the President's address. The Morganists have done everything they could to retard the development of the Michurinian theory. They have barred the way of young

scientific workers who in the past held to the chromosome theory of heredity, but who, under the pressure of experimental facts, have come to agree with one or other principle of the Michurinian theory. Professor N. P. Dubinin has particularly distinguished himself in this respect.

For instance, he spared no effort to discredit N. I. Noujdin's doctor's thesis (1944). Why did he have to do so? Because some of Noujdin's experiments with the *Drosophila* (that favourite demonstration object of the Morganists) refuted Mendelism-Morganism and pointed in favour of the principles of the Michurinian theory. Dubinin could not reconcile himself to this. At the meeting of the Scientific Council at which N. I. Noujdin defended his thesis, N. P. Dubinin went so far as to declare that if Noujdin deleted the chapter which contained the facts which did not suit him (Dubinin), he would be prepared radically to revise his opinion of the thesis. In other words, if Noujdin repudiated the facts which went against Mendelism-Morganism, he, Dubinin, would vote in favour of conferring a doctor's degree on Noujdin. Such methods of struggle are worthy only of Morganists, not of genuine scientists.

You may often hear the Morganists saying that it is impossible to discuss with Lysenko, that he "gags" critics. The Michurinian trend in agrobiological science has been, and is, a thorn in the flesh to the Morganists in our country. In the light of the effective Michurinian theory, the barrenness of Morganism stands out all too clearly. It is for this reason the Morganists cry that they are being gagged.

With the coming of Lysenko to the Lenin Academy of Agricultural Sciences, the Academy began to practice one of the great principles of science.

K. A. Timiryazev expressed the essence of this principle in the following words: "Work for science, write for the people." T. D. Lysenko is consistently practising this scientific principle. But he supplements it with another, even more effective, Leninist principle. Lenin in his time said that concrete analysis of concrete situations was the soul of dialectics.

All the researches of T. D. Lysenko and the Michurinists are subordinated to the solution of some important practical problem. This is the basis on which the Michurinian doctrine is growing and gaining in strength.

Living practice is a foe of formalism. In the light of the living practice of Michurinism, which is gathering strength in our country, the scholasticism, metaphysicism, barrenness of Morganism stand out very distinctly. This it is that gags the Morganists. They do not want to turn to living practice, which would very soon cure them of formalism. Researches into useless problems, such as those which interest Dubinin and which were mentioned in the President's address, only lead them deeper into the quagmire of formalism.

Science is a living organism which develops truth, Herzen said in his time. Soviet science is all the more a living organism because it is the science of the people. And this living, healthy organism will be able to shake off the deadening and reactionary hand of Weismannism.

The proof of this is the present session of the Academy, which bears the immortal name of Lenin, an Academy nurtured by the paternal solicitude of the great Stalin. (*Applause.*)

Academician P. P. Lobanov. I call upon Academician I. I. Prezent.

Academician I. I. Prezent. Fellow-Academicians, friends, Comrade President, at this session we are reviewing the state of biological science—the point it has arrived at, where it is heading for, and the paths it is going to take. The paths we are considering are not those of some comparatively small interval of time. Actually, the question which has been raised, and, I make bold to say, settled, here is that of the main roads of biological science for many and many decades to come.

The President, in his address, sketched a broad picture of the state of biological science, and in this connection it is appropriate, in a few words, first of all to look back on the road biological science has traversed.

If we examine the past history of biological science in Russia, we shall be struck by its very strongly marked materialist trend. There are specific reasons for this. It is explained by the very powerful influence exercised in Russia by the great enlighteners and revolutionary democrats, Herzen, Belinsky, Chernyshevsky, Dobrolyubov, Pisarev. They so prepared the public mind in Russia that the progressive intelligentsia rapidly

and easily assimilated the most progressive theory of their time, the theory of that great scientist, Darwin.

It should also be said that an analysis of the works of such eminent Russian scientists as Severtsov and Beketov shows that these scientists, by their own works and discoveries, prepared the advanced minds of Russia to assimilate, but critically assimilate, Darwin's teachings. Progressive Russian scientists had no sooner begun to assimilate the Darwinian theory than they began to weed out of it the tares it certainly does contain.

It is extremely characteristic that the eminent Russian biologists, even men who were not specialists in genetics, did not evade the root question of biology, the inheritance of acquired characters, and furnished a correct answer to this question. I. M. Sechenov, for instance, who worked in a field which would appear very remote from genetics—physiology—raised and correctly answered this cardinal question, which is the focal point of attention at this session. Sechenov wrote:

"Another factor in the continuity of evolution of the animal organism is, as we know, heredity—the capacity to transmit to offspring the variations acquired in the course of the individual life.... This feature ... is governed by the general conditions of evolution: accumulation in a continuous series of variations acquired separately by the individual members of the series, although it is attained solely by the intervention of heredity, is in reality only transmitted if the changes in the phenomena which determine the deviations from the original form continue to operate. The degree and stability of the variation is always in direct proportion to the duration of the action of the changed external influences (or conditions of life), or to the frequency with which they are repeated...."[1]

Not a geneticist, as you see, not a specialist in genetics, but a truly great materialist scientist, gave a correct answer to the fundamental question—the continuity, the inheritance of deviations individually acquired as the result of conditions of life. Are acquired characters inheritable? Is there a connection between the inherited deviations and the conditions of existence?

[1] И. М. Сеченов, „Элементы мысли," *Собрание сочинений*, том II, отд. 1, 1908 г., стр. 287.

Does the potency of heredity increase in proportion to the duration of the action of the changed external influences? To all these questions, Sechenov answers: Yes.

And in comparison with what we read in Sechenov, how sad it is to hear from our Soviet scientist, Academician Nemchinov—who, it is true, is also not a geneticist, who is a worker in the field of statistics, but who is the head of the Timiryazev Agricultural Academy—that he supports the idea of a specific hereditary substance, the idea which is the foundation of Mendelism-Morganism (Weismannism). It is precisely this foundation of foundations of Weismannism that Academician Nemchinov deems it necessary to uphold.

Today the dividing line between the Mendelian-Morganian (Weismannian) trend and its antipodes, the Michurinian trend, has been definitely defined. It is therefore extremely important to examine the attempts that have been made at this session and elsewhere to find a line of reconciliation between the two trends. Is this possible?

It should be said that a line of "reconciliation" is possible. But for this, the Morganists and those that sympathize with them, and those likewise who endeavour to justify them "statistically" (*laughter*), would have to renounce their tenet that there are two histories, one of which, the phylogenetic, is independent of the other, the history of the individual development of the organism; and that the latter in no degree determines the former. They would have to stop asserting that the development of breeds and varieties is in no wise determined by the specific mode of life and the specific conditions of development of the individual. They would have to give up the false idea of a specific "hereditary substance," having a nature of its own, distinct from that of the body, and upon which the rest of the body has no influence, no specific effect. They would have to give up the theory that the gamete is pure and retains its purity unaffected by the influences proceeding from the body and the conditions of its life. They would have to give up considering this mythical "hereditary substance" as consisting of separate, isolated elements, located in this substance and only able from time to time to join together and translocate themselves, while at the same time retaining their absolute unchangeability and mutual independence.

To put it briefly, in order that the Morganists might be "reconciled" with the Michurinian theory, they would have to renounce every one of the theoretical concepts of their false doctrine. Michurinian science, the Michurinian biology, will not allow itself to be inveigled into any other channel of reconciliation. Nor will it be inveigled by those who try to falsify the Michurin theory itself, to repaint and adjust this progressive theory to reactionary Morganism, in order then to proclaim themselves adherents of the Michurinian trend.

In our country there are now very few outspoken Morganists. For this one must really be a Dubinin (*applause*); and if you were to ask me who nowadays is most dangerous to the progress of the Michurinian theory—Dubinin, or Zhebrak, or their ilk—I would reply: no, the most harmful to the Michurinian cause today are those who smuggle in anti-Michurinian, Weismann-Morganian views under the guise of sympathy for Michurin, people of the type of Zavadovsky and Alikhanian.

Voices. Hear, hear!

I. I. Prezent. I shall take the liberty first of all to analyze the attempts made at this session and elsewhere to dress the Michurinian theory in a Morganist guise, such as are made, for instance, by Alikhanian. The device he resorts to is not original, and consists in the following. Michurin opposed Grell. Grell believed in the influence of the stock on the scion. But Michurin was against Grell. Consequently, Alikhanian concludes, Michurin did not believe that the scion influenced the stock, did not believe in vegetative hybridization.

To this I would say to Comrade Alikhanian, you are committing an elementary logical error, my dear "Neo-Michurinist"—and an error not so difficult to detect as to permit you to get away with it unnoticed. You want to persuade your hearers and readers that, since I. V. Michurin did not agree with Grell's idea of the influence of the stock on the scion, he therefore did not believe that the stock had any influence on the scion in general. Actually, the sort of thing you are trying to have people believe is that, since all birds are bipeds, all bipeds are birds. But there is man, who is a biped but not a bird! Your logic will not work, Comrade Alikhanian. We shall not allow this species of "logic" through the gates of the Michurinian theory.

In order in one way or another to substantiate his monstrous misrepresentation of Michurin's views, Alikhanian cites Michurin's criticism of statements made by Grell's follower, Cherabayev. Let us follow Alikhanian in quoting what Michurin said, and examine it.

"Lastly, I cannot understand," Michurin wrote, "why the editors did not think it necessary to make any comment on Mr. Cherabayev's article regarding the influence of the stock on the grafted variety. Just examine it, please, there is certainly something very incongruous about it. In his opinion, the stock has an influence on positively every part of the variety grafted on to it: on growth, on fruit bearing, on the shoots, on hardiness and, lastly, on the formation of the seed, yet, suddenly an unexpected exception: when it comes to the quality of the fruit, he does not recognize this influence as operative. Say what you like, but it is hard to agree with this. All the more that this is not the way it works out in reality."[1]

How does it work out in reality, according to Michurin?

Everyone who has the slightest acquaintance with Michurin's work knows that his theory had its beginnings in his discovery of the fundamental difference between the young seedling which has only just begun its cycle of development, and the old plant which has already borne fruit many times and whose characteristics are well established. The young plant, according to Michurin, is capable to a greater degree than the plant which has fruited many times of yielding to every sort of influence, including the influence of the stock or the scion. After his discovery, Michurin fought, not the idea that the scion influences the stock and the stock the scion, but Grell's mistaken views, which did not distinguish between young and old plants, and which assumed that all plants and every part of them were equally susceptible to the influence of scion or stock, regardless of whether the object of the influence is younger in development, in respect to fruiting, than the variety grafted on to it. This was Grell's fundamental error, as a result of which he was never able to perform vegetative hybridization. But Michurin, having detected the reasons for Grell's failure, discovered the law of the reciprocal

[1] И. В. Мичурин, *Сочинения*, том I, стр. 143.

influence of scion and stock, thereby at the same time laying the foundation of the theory of plant development. And when Michurin found in Grell's or in Cherabayev's writings that, in their opinion, when old varieties were grafted together, all characters changed except the quality of the fruit, which supposedly remained unaltered, Michurin could not help protesting. For one or the other: either the old variety, which has long and repeatedly fruited, is susceptible to no influence on the part of the young seedling on which it is grafted; or, if a young tree is grafted on another, older tree, which has repeatedly borne fruit, then the young graft will undergo changes also with respect to its fruits. It was on this basis that Michurin elaborated a whole system of vegetative hybridization, which includes the system of the mentor. And after the brilliant results he achieved, Michurin summed up his numerous experiments in vegetative hybridization in the following words:

"That vegetative hybridization is undoubtedly possible, is a question which I consider definitely settled."[1]

Whom in this audience did Comrade Alikhanian hope to lead astray? Could he have forgotten that he is not at a conference of the Moscow University, where speeches of this sort might have been greeted very sympathetically and have earned Alikhanian the laurels of an expert on the Michurinian theory and a refuter of Lysenko? In this audience, however, at this session of the Agricultural Academy, Alikhanian's statements can only be regarded as a specimen of howling ignorance, or as a plain falsification of Michurin's teachings.

I myself am inclined to regard Alikhanian's speech, which reiterates his recent Lomonosov Reading, as a repetition of his attempt to falsify Michurin's ideas in deference to Mendelism-Morganism. It is a hopeless attempt, for it is incomprehensible whom Alikhanian thought of leading astray in this auditorium, where people are gathered who have made Michurin's teachings the working method in their researches.

Aware that the situation is not what it was, the anti-Michurinists are now endeavouring to turn Michurin himself into an anti-Michurinist, to turn him into a Mendelist. How is this

[1] *Ibid.*, p. 277

done? The operation is performed as follows. The first volume of Michurin's works is opened, where he expounds his principles and methods, and the following passage is quoted:

"When investigating the application of Mendel's law to the hybridization of cultivated varieties of fruit plants, I recommend that, for a beginning, the investigation should be confined to observing the hereditary transmission of one of the two characters, just as Mendel himself did in his work on peas. I find it particularly useful to indicate a few of the best and in every way exemplary experiments in hybridization.... On the basis of my own work, I would recommend to use for these exemplary hybridizations the following pairs: of the apples, Malus Niedzwetzkiana is good as the male component, and as the female component the following cultivated varieties might be indicated: Anis and its variants, Korichnoye, Kandil Sinap, Chelebi," etc. Having recommended the apple varieties for the exemplary hybridization, Michurin remarks: "Here there is great scope for applying the whole Mendelian calculus to the entire complex of characters of each hybrid."[1]

Quoting these words of Michurin, our Mendelists exclaim: There, you see, Michurin himself says that there is great scope for applying the whole Mendelian calculus—with all its statistical adjuncts, which so arouse the admiration of some of our statisticians. Is it not clear that Michurin was a Mendelist?

However, our Mendelist-Morganists must think people extremely naive and credulous. A careful study of all Michurin's works will show that what he had in mind in the passage quoted was the very opposite of what the Mendelists ascribe to him. Michurin recommends that, for pedagogical purposes, the crossing should be made between such pairs as would unreservedly convince everyone who is capable of weighing the facts that the Mendelian scheme is worthless.

In 1914-15, in Michurin's orchard, hybrids obtained from a cross between the well-known Antonovka apple and the Niedzwetzkiana bore fruit. All the organs of the Niedzwetzkiana—bark, leaves, etc.—are of a red colour. Michurin had impatiently awaited the fruiting of these hybrids, for already in 1912 he had pointed out: "In general, the Pyrus Niedzwetz-

[1] И. В. Мичурин, *Сочинения*, том. I, стр. 343-44.

kiana hybrids are extremely convenient for the observation of the union of characters and properties of plants in hybridization, since the colour of the bark, leaves and wood, as well as of the blossoms and fruit, extremely facilitate observation."[1] The sharp contrast in the colour of all the organs of the parents makes this cross extremely convenient as an exemplary experimental object for testing the Mendelian scheme. Here, indeed, there is great scope for applying the entire Mendelian calculus.

The results obtained by Michurin from this cross very graphically demonstrated that in this case—where there is full scope for the Mendelian calculus of the distribution of characters in the progeny—the Mendelian scheme is utterly refuted. The hybrid progeny behaved anything but à la Mendel. It is sufficient to mention, for example, that among the hybrids there was one which was tinted red on one side and green on the other. This "chimera" alone, obtained from sexual hybridization, and similar to the "chimeras" which are not infrequently obtained by vegetative hybridization, refutes the Mendelian scheme. This fact, as well as others, also of an anti-Mendelian character, were noted by Michurin: "There are also vegetative hybrids of this kind. In general, the Pyrus Niedzwetzkiana hybrids are extremely convenient for observation of the union of characters and properties of plants in hybridization."[2]

Describing the behaviour of Niedzwetzkiana hybrids in a letter to the well-known horticulturist, Pashkevich, Michurin remarked:

"All this is very interesting because observation of the appearance of the fruits of the hybrids themselves, and especially of the seedlings obtained from their seeds, is the easiest way of all to demonstrate the unsoundness of Mendel's pea laws."[3]

From that time on Michurin never ceased to recommend crossing with the Niedzwetzkiana apple as a means of proving the incorrectness and fallaciousness of the Mendelian pea laws.

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[1] *Ibid.*, Vol. III, p. 222.

[2] *Ibid.*

[3] *Ibid.*, Vol. IV, p. 237.

He repeats his recommendation in his "Materials for Elaborating Rules of Training," where he writes:

"Very interesting and in the highest degree useful from the scientific standpoint are the crossings I made of several cultivated varieties of apple with the long-known red-leafed Niedzwetzkiana apple. The seedlings obtained from the hybrids, from the earliest moment of their development from the seeds, made it possible to see and observe the gradual development of the hereditarily transmitted characters of the parent plants to their offspring by the different degrees of red colouring of all the parts of the seedling, beginning with its cotyledons and ending with other parts when they are completely developed in the plant at a riper age. All this can easily be seen even by the most inexperienced observer, by a neophyte in hybridization, owing to the red colouring of the leaves, shoots, bark and wood, the similar colouring of the roots, blossoms, rind of the fruits and, lastly, by the colouring of the very flesh of the fruits and their seeds. In addition, by these observations one can most easily, quickly and, chief of all, surely demonstrate the utter unsoundness and inapplicability to the hybridization of fruit plants of the celebrated Mendelian pea laws, which have been so insistently recommended to us by our horticultural scientists, who actually have proved to be utter ignoramuses in the matter of hybridization. Any amateur horticulturist may easily convince himself of the correctness of this conclusion of mine if he repeats my experiments in crossing the Niedzwetzkiana with any cultivated variety of apple in his own orchard."[1]

And when Michurin summed up the results of his sixty years' work, he advised everybody to prove to himself the complete unsoundness of Mendel's laws by taking such convenient objects as Niedzwetzkiana hybrids, where from the moment the seedlings appear one may observe whether the characteristics deviate towards the paternal side or the maternal side. They are no less, and, in fact, more convenient for demonstration purposes than peas; and the results will make the utter fallaciousness of Mendel's pea laws apparent to everyone.

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[1] И. В. Мичурин, *Сочинения*, том I, стр. 261-62.

This, as Michurin saw it, was the pedagogical significance of the exemplary hybridization he recommended in investigating the application of the Mendelian laws. Essentially, what Michurin says here is this: do you want to test the whole Mendelian calculus and to have a convenient object for the purpose? Then take for your crossings the Niedzwetzkiana apple, it is more convenient than peas, and will open the eyes even of those who make statistics a new speciality for the sanctification and for the defence of the Mendelian schemes. (*Applause.*)

It need not be said that Michurin's doctrine has been richly developed in the work now being done by the large and competent body of Michurinists, who are proud of the fact that they are headed by T. D. Lysenko—a masterly experimenter and subtle thinker. This trend has in a very short period tackled and solved in the most general, and at the same time most concrete, form many problems of biological and bio-agronomical science, and has made no small contribution to the national economy. It is impossible to enumerate all the problems that have been solved or are in process of being solved. However, I shall take the liberty of dwelling on one of them, namely, vegetative hybridization.

Academician Zhukovsky declared here that he has never seen a single documentarily demonstrated fact of vegetative hybridization. But, in advancing this thesis, Academician Zhukovsky securely guaranteed himself against all facts. You know, Academician Zhukovsky, it is impossible to demonstrate to you facts of vegetative hybridization, since you have in advance verbally annihilated all such facts by declaring: anything you show me in the way of vegetative hybrids I shall call mutations anyhow. Of course, you can call things at your own sweet will; you are the full master of your words. However, you are a bad master of their meaning. Surely, you cannot, by simply pronouncing the word "mutation," annihilate vegetative hybrids which bear the clear and distinct characters of their parents.

T. D. Lysenko. Tomorrow I shall demonstrate here a score or so of vegetative hybrids which are absolutely similar to sexual hybrids, you will be unable to distinguish them from the latter. What is more, they have already been in existence for fifteen years. The only people who have not seen them are

Dubinin and those who live next door, in the Academy of Sciences, where these hybrids are housed.

I. I. Prezent. You see how good and useful for the elucidation of a question it sometimes is to interrupt the speaker. (*Laughter and applause.*)

Well, tomorrow Academician Zhukovsky will have another opportunity to engage in verbal exercises, to dub the phenomenon and concept of vegetative hybridization with the Latin term "mutation." True, Academician Zhukovsky will have to dub as "mutations" such vegetative hybrids as deviate towards one or other of the parents, as is in most cases to be expected of hybrids. But it is hardly creditable for a scientist to confuse phenomena and concepts solely because of a stubborn, wilful and absolutely unwarranted predilection for Morganism.

P. M. Zhukovsky. And what if pollination occurred here?

I. I. Prezent. Academician Zhukovsky interrupts and asks: was not the effect of vegetative hybridization actually due to unforeseen pollination by another variety; has there not been an elementary error here, a sexual hybrid having been obtained which has been called a vegetative hybrid? This is the usual argument and objection levelled against the Michurinists by the Morganists. I make so bold as to assure you that the Michurinists are far more experienced and subtle experimenters than the Morganists, and that the possibility of such an elementary error was of course foreseen and averted. The argument of outside pollination advanced in refutation of vegetative hybrids is just as weighty as the argument of the foreign biologists hostile to us who assert that all Lysenko's work is unsound because, as they claim, everyone knows that the Russian varieties are impure. That is what is said, for instance, by Hudson and Richens in their lengthy treatise, *The New Genetics in the Soviet Union*. But, you will agree that this argument *ad impurum* shows they must be in a pretty bad fix. (*Laughter and prolonged applause.*)

Academician Zhukovsky asks us: where are these vegetative hybrids to be found? Where, one wonders, was Academician Zhukovsky and the other supporters of Morganism when the Michurinist, Isayev, demonstrated from this platform his remarkable hybrid—the sexual offspring of a back cross with

Michurin's vegetative hybrid apple Reinette-Bergamote? Some fifty years ago, Michurin grafted an ordinary apple on a pear. The graft was only a short time on the roots of the pear, then Michurin grew it on its own roots and for several decades propagated it vegetatively. Thus Reinette-Bergamote, having already become a fully established variety, was grown on the roots of the most diverse varieties, but it did not lose the character of the pear shape of its fruits which it had acquired by vegetative hybridization. And when, fifty years after Michurin had grafted Antonovka on a pear stock, the Michurinist, Isayev, back-crossed Reinette-Bergamote with the Pepin Shafranny apple, and moreover took the vegetative hybrid as the mother and the apple tree as the father—that is, did everything, it would seem, so that not even the ghost of the vegetative hybrid remained in the sexual progeny—what did we find? Did the "ghost" of the pear stock remain?

Well, hybrid trees obtained from recrossing the vegetative hybrid with the apple have been fruiting for the fourth year, yet the hybrid continues to bear pear-shaped fruits. The only way to contest this incontestible fact is to declare that although the fruits produced by the apple tree do look like pears, nevertheless it cannot be.

P. M. Zhukovsky. Don't you know of a wild apple with pear-shaped fruits?

I. I. Prezent. But in this case the cross was made with a cultivated apple, not a wild one. And have you ever seen a wilding being obtained from grafting one cultivated variety on another cultivated variety? True, you might object that this sometimes does happen with sexual crosses. I accept the objection as a possibility, but then it speaks in favour of what I am arguing. For if one accepts the objection that as a result of the graft a wilding turned up with pear-shaped fruits, this only shows that vegetative hybridization may have the same effect as sexual hybridization. No, Academician Zhukovsky, you have no escape. And you won't think one up, I can guarantee you that!

They say there are no such things as vegetative hybrids. But are you aware, Academician Zhukovsky, of the case that happened in the Timiryazev Academy? Some of the workers at the Academy, in spite of the hostility to the Michurinian

doctrine and methods inculcated there (incidentally, Academician Nemchinov, let me tell you that there are Michurinists in your Academy; and I can console you with the thought that there will be more of them soon) (*laughter*)—certain Michurinists in the Timiryazev Academy performed a vegetative hybridization and sowed out the seeds of the hybrids in the grounds. Since they were vegetative hybrids of the tomato and stramonium, a warning sign was put up: "Beware, don't pull the fruits. Dangerous." Nevertheless—not from disbelief in vegetative hybrids, of course, but simply from ignorance—some passers-by did pull the fruits and eat them—and ended up in the hospital.

If these facts are not enough, I might remind you that not long before the war, at the Academy's experimental base at Gorki-Leninskiye, graftings of tomatoes were made on solanum. As a result of the grafting the fruits not only of the hybrids but also of their seed progeny acquired a rather pleasant piquant flavour. The seed progeny obtained from these vegetative hybrids were so abundant that their fruits were sent to the local cooperative store for sale. People from the surrounding villages, who apparently got to like the piquant flavour of the hybrid fruits, used to come to the store and simply say: "Weigh me a kilogram of vegetative hybrids." These were plain common folk, who appreciated the tomatoes for their taste, and not for their name. Very likely, here too Academician Zhukovsky would find a way out of the predicament and, when buying vegetative hybrid tomatoes, would say: "Give me a kilogram of mutations."

The Morganists retail many fables about the Michurinists. They use this weapon for lack of anything better. For instance, Academician Zhukovsky said here that he had heard from someone that Lysenko and his followers had declared that the discovery by the Russian scientist, S. G. Navashin, of double fertilization leading to the formation of endosperm was a delusion.

I ask you, who is developing the progressive side of S. G. Navashin's work? What have the Morganists done to theoretically explain the phenomenon and effect of double fertilization from the Darwinian standpoint? Nothing. But we, modest Michurinists, have taken up and are studying this problem

from the angle of the theory of biological progress elaborated by another eminent Russian scientist, A. N. Severtsov, from the standpoint of the Darwin-Michurinian theory of the value of fertilization in general, and of cross-fertilization in particular. I shall take the liberty of stating briefly here that, working experimentally on this subject, I have come to the conclusion that double fertilization has a biological significance similar to that of fertilization in general. Thanks to double fertilization, nourishment of a special kind is obtained, in which is included a wealth of specific adaptation. Double fertilization, like fertilization generally, has the effect of broadening the adaptive potentialities of the organism, of widening the amplitude of its connections with external conditions, while at the same time the developing organism is morphologically less dependent upon fluctuations of its external environment. This is precisely that general enhancement of vitality which Academician A. N. Severtsov defined as "aromorphosis." The effect of this type of aromorphosis is the opposite of inbreeding. And when we, in our experiments, removed the product of the double fertilization, that is, the endosperm, and grew the plant from embryos isolated from the endosperm, we obtained something analogous to inbreeding: the plants of stable equalized strains proved to be extremely diverse, with changed characters, even with changes of variety. This is true both of non-hybrid, and even more of hybrid forms. As to the latter, I shall here reply to another assertion of Academician Zhukovsky, namely, that he knows of no exceptions to the Mendelian rules. He would be much closer to the truth if he said that he knows of no corroboration of these rules. As to the exceptions, permit me to exhibit this. (*Exhibit.*) You said, Academician Zhukovsky, that the Mendelian rules were firmly established.

P. M. Zhukovsky. For annual self-pollinators.

I. I. Prezent. Permit me to exhibit hybrid wheat plants obtained from crossing an awnless pubescent variety with an awned glabrous variety. As you see, this is a self-pollinator and, moreover, an annual. Well, be good enough to tell me, which generation is this? According to you, and according to Mendel, this cannot be the first, for here we have in one and the same generation awned and awnless forms, pubescent and glabrous. But this is the first generation. And in order to obtain

such a "segregation," it was sufficient to grow the hybrid plants of the first generation from embryos isolated from the endosperm, to deprive them of hybrid nourishment.

Comrades, our Mendelist-Morganists are now trying to pass themselves off as Darwinists, sometimes calling themselves genuine Darwinists, and also orthodox Darwinists. And, of course, one of the first to be proclaimed such a Darwinist is Academician I. I. Schmalhausen.

I have long expected that if Academician Schmalhausen had any remarks or denials to make with reference to my analysis that his work is anti-Darwinist, he would make them openly to the scientific public. Yet two years have passed, and Academician Schmalhausen, while not coming out in the scientific press on this subject, keeps writing complaints everywhere asserting that Prezent is misrepresenting him. But such complaints cannot replace scientific argument. At last, after long effort, Academician Schmalhausen was persuaded to speak here from this scientific platform.

A voice. But he is sick.

I. I. Prezent. Sick for two years? If he is too sick to write a refutation, how is it that he is well enough to write complaints about me? (*Laughter.*)

Academician Schmalhausen declared today that he is a genuine Darwinist, and that it is impossible to find any departures from Darwin in his works.

In order to analyze Schmalhausen's mistakes fully, one would have to quote his works line for line. I am unable to do that, and I shall therefore only touch upon a few of his erroneous views.

Let me point, to begin with, to the following paradox. Schmalhausen proclaims himself a foe of Lamarck. The bourgeois idealist biologist, Cope, is one of the outstanding pillars of idealism in biology, who takes from Lamarck all that is most unsound and most backward and discards the progressive and materialist aspects of the Lamarckian theory. As we know, Cope proclaimed the so-called "doctrine of the unspecialized."

The essence of this doctrine is that a new form can have its origin only in living beings with unspecialized organizations, and that the farther the process of specialization of the organ-

ism and its characteristics has proceeded, the less chance there is of new forms appearing. From this "doctrine" the followers of Cope have drawn appropriate conclusions. They ask: Can man, for instance, have descended from some ancestor of the present-day ape? And, having examined all fossil apes, they show that every one of them has some degree of specialization. Of course, the seas and continents were never inhabited by diagrams, they were inhabited by organisms which were adapted, specialized in one degree or another. And if the "doctrine of the unspecialized" is true, then man cannot be regarded as having descended from any of the fossil apes, but must have had his own sources of origin, independent of all other animals. It is not difficult to see that this sort of argument is utterly anti-Darwinian, and patently leads to religious obfuscation.

Our eminent Russian scientists, men like Academician Sushkin, refuted Cope's false and unscientific doctrine. But Academician Schmalhausen, in spite of all its absurdity, accepts it. And Schmalhausen lauds and extols Cope in his book, *Problems of Darwinism*. And this book Schmalhausen recommended from this platform as fully Michurinist and Darwinistic.

This is what Schmalhausen writes.

"... We must turn our attention to the data of paleontology mentioned by Cope and since repeatedly confirmed.... Consequently, new forms always derive from poorly specialized ancestors—representatives of an earlier epoch.... These conclusions follow from the sum-total of our knowledge."[1]

I should like to ask Academician Schmalhausen, who believes in the Cope doctrine, whether he considers that the modern penguin, which has a less specialized tarso-metatarsus than the archaeopteryx, cannot have descended from it, but must have had a primeval progenitor of its own, different from that of all other birds. How does Schmalhausen explain that evolution is still continuing among modern plants and animals, which are already quite fairly specialized: or does he believe with Julian Huxley in the end of progressive evolution, which now, according to Huxley, hangs on a single slender thread, the

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[1] И. И. Шмальгаузен, *Проблемы дарвинизма*, стр. 465.

evolution of man? I ask, how can Cope's nonsense be regarded as a useful contribution to Darwinism? I ask, how can an anti-Darwinian concept be retailed in a book which bears the title *Problems of Darwinism*? I ask, how can such a book be recommended by the Ministry of Higher Education as a manual for our universities?

Schmalhausen declared that all the errors ascribed to him are ascribed to him falsely. Well then, permit me to ask, does he still consider the following passage in his book, *Problems of Darwinism,* correct?

"After the foundations of modern genetics were laid in the form of Mendelism, the Swiss scientist Johannsen set out to verify the theory of natural selection experimentally. He found that in the case of the population of self-pollinating plants (beans) artificial selection leads to the production of pure lines, which then remain constant. Within pure lines selection is found to be impotent. These facts were mistakenly interpreted as contradicting the theory of natural selection.... There are no pure lines in nature and selection always operates in more or less heterogeneous populations, possessing an enormous range of diverse individual characters. In this case, natural selection has a practically unlimited field of action." (P. 204.)

It is not difficult to see that Schmalhausen considers the assertion of the Morganists that selection is inoperative in "pure lines" inapplicable to elemental nature solely for the reason that in nature there are no "pure lines." But Schmalhausen does consider this assertion quite appropriate and correct where there are "pure lines," that is, in the realm of breeding. It is to be wondered where Schmalhausen was all these years, in what regions his thought hovered, if he, who considers himself a Darwinist, is quite unfamiliar with the facts about breeding, if he does not know that thousands of people have experimentally confirmed T. D. Lysenko's views on the variability of pure lines, if he does not know that wholesale experiments on intravarietal crossing, including crossing within pure lines, have yielded undoubted and, moreover, positive results. Is it creditable of a professor of Darwinism to assert that within pure lines selection is impotent?

In general, I. I. Schmalhausen is to be sympathized with. A teacher of Darwinism, he has very little relation with the Darwinian theory, and if he has, it is very negative. But, on the other hand, if you have taken up some field of knowledge, and bear the title of an academician, you should be mindful of what Academician Zhukovsky said from this platform, that "knowledge is light, and ignorance darkness."

Academician Schmalhausen declared that he is misrepresented, when he is accused of asserting that mutations are indeterminate. More, in a recent issue of *Vestnik Moskovskovo Universiteta,* Academician Schmalhausen is portrayed as a champion and defender of the idea that hereditary variations are determinate, that they are qualitatively dependent on the conditions of life.

I do not know under the influence of what external conditions the biological faculty of the University has begun to present Schmalhausen in this light. But you can't get away from facts, from documents. And after all, Schmalhausen, voicing his solidarity with Darwin, did write: "According to Darwin, individual variability could not have played any leading role in evolution, since it has an initially indeterminate character; it has no directiveness." (P. 190.) Schmalhausen did write that "it should not surprise us therefore that by applying definite factors we get various mutations, and that by the action of different agents we in general get the same mutations as occur in nature." (P. 221.) Schmalhausen did assert that "indeterminateness of the reaction, together with hereditability of the variations, is the best description of mutations." (P. 210.)

Schmalhausen declares that he is misrepresented when the assertion is ascribed to him that variability declines with the process of evolution.

But it was Schmalhausen, wasn't it, that declared that "allomorphosis passes quite naturally into telomorphosis, i.e., into specialization, which is associated with loss of plasticity and gradual dwindling of evolution" (p. 497), and that "the individual variability of the organism will continuously decline. A specialized organism loses its plasticity" (p. 506).

Schmalhausen declared from this platform that he had never and nowhere asserted that at the dawn of their introduction to cultivation animal and plant species are much more

richly endowed with variability than subsequently; that cultivated species of animals and plants reveal a descending curve of variability. He says that he made certain reservations which limit his assertions. But the reservations do not alter Schmalhausen's basic line of argument. He declared at this session, too, that the beet, for instance, underwent profuse variations at the dawn of its introduction to cultivation, but that later its variability steadily declined. Who then, I ask, is the misrepresenter? Is it not Academician Schmalhausen himself who appears in this unseemly role, when he misrepresents the phenomena and laws of nature and, in the endeavour to justify himself, misrepresents the meaning of his own assertions.

I. I. Schmalhausen and his defender, I. M. Polyakov, declare that while it is true that *Factors of Evolution* contains no mention of Michurin and the Michurinists, the *Problems of Darwinism,* on the other hand, is full of these names, is full of the ideas of Michurin and his followers. Yes, indeed, in the latter book, in contradistinction to the former, the teachings and methods of Michurin and Lysenko are expounded. But how are they expounded?

"The Michurin methods of 'training,'" writes Schmalhausen, "imply the creation of such conditions for the development of the organism as will facilitate the maximum elicitation of characters useful to us."[1]

Consequently, mentors and other methods of training nonhybrid and hybrid forms are represented by Schmalhausen to our students only as methods of eliciting characters, and not of creating them. This is a deliberate and gross distortion of the Michurinian teachings. Michurin long ago rejected the interpretation of the role of external conditions as merely that of bringing out pre-existing characters. "That is wrong," protested Michurin against assertions of this kind. "Not all rudiments of characters are latent in the gametes. Some may evolve and manifest themselves under the influence of factors of the external environment, among which may be classed those hereditarily induced by man by grafting a plant on to a stock of another variety."[2]

[1] И. И. Шмальгаузен, *Проблемы дарвинизма*, стр. 241-42.
[2] И. В. Мичурин, *Сочинения*, том IV, стр. 196.

Schmalhausen expounds the work of Academician T. D. Lysenko in similarly garbled form. He tries to have the reader believe that Lysenko succeeded in obtaining only modificational, that is, non-hereditary changes of organisms—and not in directively altering the very nature of organic forms, not in deliberately and systematically changing their heredity. "The theory of Academician Lysenko," writes Schmalhausen, "supplements this with the demand for specific environmental conditions also at every stage of development, corresponding with the characteristics of each given variety. This represents a further step in the governing of the individual, i.e., the modificational variability of organisms."[1]

In his methods of misrepresenting Michurin and Lysenko, Schmalhausen keeps in step with his like-thinkers and followers. Since it is impossible to avoid saying anything about Michurin in a book intended for Soviet students, Michurin's methods are represented as means of eliciting future characters already determined in the embryonic cell; and Michurin himself is represented, not as a great transformer of nature, but in the shabby role of an "elicitor." A similar operation is performed in the case of Lysenko. Lysenko, it is claimed, only directs modifications, and nothing more. He may vernalize winter varieties, but not convert them into spring varieties. Did not the young Morganist, Lobashev (now promoted, for his Morganistic services, to the post of dean of the biological faculty), once assert that Lysenko converts hereditary winter varieties into non-hereditary spring varieties? Devices of this kind are extremely characteristic of the way the Morganists expound the teachings of Michurin and Lysenko. No, it would be better if the followers of Schmalhausen maintained silence about Michurin and Lysenko, as Schmalhausen himself does in his *Factors of Evolution*. Distortion is more harmful than silence.

Of Academician Schmalhausen and his dogmas much could be said and should be said. But time is passing, and I shall therefore dwell only on one other point in the speech he made today at this session. Schmalhausen declared that he was Severtsov's successor in his work, and that Severtsov himself

[1] *Ibid.*, p. 242

anointed him as his successor. I do not venture to dispute this. But if Severtsov did anoint Schmalhausen, then it must be admitted that the anointed has not justified the very honourable dignity conferred upon him. It may in truth be said that Academician Schmalhausen, under the guise of "continuing" Severtsov's work, only multiplies and classifies words, pretending that he is developing Severtsov's theory, when he is only cluttering it up with allomorphoses, telomorphoses, katomorphoses, hypomorphoses, hypermorphoses—and even epimorphosis, by which is meant human history placed in the general order of classification of lines of animal evolution. Henceforth, let our teachers of historical materialism know that the history of human society is—epimorphosis!

The basic idea of Schmalhausen's worthless and unscientific theories is Weismann's idea of the autonomous organism, which has been clearly expounded by Schmalhausen's follower, Professor Paramonov, who declared that "the organism constitutes an independent system, and the surrounding environment another system. . . . The direction of changes in the environment and of the variability of the organism are independent of each other."[1]

Our Morganists, retreating all along the line in face of the pressure of Michurinist facts, are trying to make a stand on a front which is least subject to the attack of the Michurinists. This front is cytology. It was on this cytological charger or Paradepferd, as the Germans call it, that Academician Zhukovsky entered the lists today.

But you, Academician Zhukovsky, are a botanist, specializing in the field of fine structures. You ought to know that the days of Boveri and Strasburger are in the pluperfect, as Alikhanian once put it in reference to Academician Schmalhausen, after his, Schmalhausen's, book was not entered for the Stalin Prize. How can you, a botanist, fail to know that there is now a great deal of most delicate work in cytology, resulting from the use of new microscopes and new reagents, which completely refutes the entire cytogenetic scheme? And if you are familiar with this work, why do you conceal it and not draw the appropriate conclusions from it?

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[1] А. А. Парамонов, *Курс дарвинизма*, 1945 г., стр. 253-54.

The Soviet scientist, Professor Makarov (Leningrad University) has demonstrated that the so-called continuity of the chromosomes is a myth. Jeffrey, an eminent cytologist, has shown that one of the fundamental tenets of cytogenetics, which Academician Zhukovsky tried to demonstrate here, the tenet that the reduction in chromosomal number in the meiotic stage is merely the result of the fusion of the chromosomes side by side in pairs as reduction or meiosis takes place—that this tenet is untrue. "This assumption," Jeffrey writes, "is the result of a palpable disregard of the structure of the somatic or bodily chromosomes and, above all, of the organization of the hitherto completely structurely unknown reproductive or gametic chromosomes. In the actual state of our knowledge there is no authentic case of the union of chromosomes side by side, for a study of the organization of all types of chromosomes shows clearly that chromosomes become united invariably only end to end. Moreover, the union of chromosomes never takes place at the beginning of a nuclear division (as is assumed in current speculations as to the nature of meiosis or reduction), but at the end (telophase) of the immediately preceding division. It may be added that not only is the union of chromosomes in cell division characteristic of, and confined to, the end of the preceding division, but this situation is likewise an unfailing feature of all chromosomal unions, whether somatic, reproductive, or reductional."[1]

Having described and demonstrated the structure of chromosomes with the help of perfected photomicrographs, Jeffrey says that in all the varied types of chromosomes of the soma and of the gamete the organization is identical. "This situation," he concludes, "as has been shown objectively above by photomicrographs, is entirely unfavorable to the current views regarding the relation of chromosomes to inheritance. . . . It follows that the doctrine of the side-by-side, or lateral, fusion of chromosomes in the reduction division and the conclusions in regard to hereditary transmission based on that supposed condition have no sound basis in fact."[2]

Jeffrey's discovery induced even the editors of such a jour-

[1] E. C. Jeffrey, "The Nucleus in Relation to Heredity and Sex," *Science*, Oct. 3, 1947, p. 306.

[2] *Ibid.*, p. 307.

nal as *Science* to declare that these investigations "render necessary a revision of our views regarding the relation of the nucleus and its derivative chromosomes to heredity and the determination of sex."[1]

Franz Schrader, Vice-President of the American Association for the Advancement of Science and Chairman of its Section of Zoological Science, reviewing three quarter-centuries of cytology, feels compelled to admit that "in the cytology of *Drosophila* itself there is much that does not conform to what we have set up as the standard course of events,"[2] that "much of the foundation on which modern cytogenetics has been erected is in need of reconstruction," and that "almost all cytologists, except for Darlington and his followers, are now convinced that the factual findings are erroneous."[3]

The Morganian geneticists usually assert, as something which irrefutably confirms their system of views, that the problem of sex is now indubitably solved on the basis of Morganism, on the basis of the so-called X-Y chromosomes. But read the works of Morgan himself—only not the popular publications for students and the general public, but those intended for his fellows in thought, and you will find that Morgan has to admit that sex cannot be regarded as determined solely by the presence of the X and Y chromosomes.

The cytogenetic system is crumbling. No wonder the Morganists are thinking up, in addition to the gene, all sorts of "plasmogenes," "plastidogenes" and similar terms designed to veil the complete theoretical and factual discomfiture of Morganism. No wonder Hugo Iltes, the man who, when a monument was erected to Mendel, vowed fidelity to his ashes—thereby anticipating the recommendation of Academician Zhukovsky—now, having become the curator of the Mendelian Museum in Fredericksburg, sadly observes:

"Genes have fallen on evil days. It is hard to be doubted out of existence.... It is hardly less an offence against the dignity of a gene whose main pride has been its crystal-like

[1] E. C. Jeffrey, "The Nucleus in Relation to Heredity and Sex," *Science*, Oct. 3, 1947, p. 305.

[2] F. Schrader, "Three Quarter-Centuries of Cytology," *Science*, Feb. 13, 1948, p. 159.

[3] *Ibid.*, p. 158.

constancy and purity, to be accused of being labile, and hardly more stable than a lump of sugar melting in a cup of coffee."[1]

Mendelism-Morganism has already betrayed its utter hollowness; it is also rotting from within, and nothing can save it.

Comrades, let us recall a little history. About ten years ago, in this hall, two ideologically antithetical conceptions clashed—Morganism (Weismannism) and the Michurinian doctrine. At that time Academician Serebrovsky declared from this platform that we were on the threshold, on the verge, of great discoveries, and he pleaded for a little time for the accomplishment of these discoveries. The Morganists have traversed a ten-year path, and we now hear the same statement from Rapoport, another Morganist who has grown up in the meantime. But the adherents of the Michurinian trend have come to this session with major achievements, which enrich our practice. In this period the Michurinists have again demonstrated the possibility of directing evolution. They have shown that, in the hands of the investigator, the Michurinian trend gives, if not two birds in the bush but only one in the hand, yet a bird that has actually been caught: the possibility of directing the evolutionary process.

Having no achievements of their own to their credit—save for a few isolated ones, obtained accidentally—our Morganists are trying to appropriate the work of Academician Lisitsyn, of Academician Konstantinov, of Dr. Shekhurdin and the entire host of our plant breeders. What should we say, for example, of the device resorted to by B. M. Zavadovsky, who, in his zeal to defend Morganism, when speaking of the use of colchicine as a method of plant breeding, appeals to the work of Shekhurdin? The plant breeders sitting here, I saw it myself, simply smiled at this. Shekhurdin, Konstantinov and Lisitsyn used a sound and productive method. It is the method of selection. It has not been removed from the order of the day—it is an old Darwinist method. It is true, Academician Konstantinov, that in these Michurinian days, the days of systematic plant breeding, including training, the old method of selection is not enough. However, this old method, which was expounded in the works of Darwin, bears no relation either to the Mendelian count of the number of character segrega-

[1] *The Journal of Heredity*, 1944, No. 11, p. 347.

tions in the direction of the father or the mother, or to colchicinization, that banking on an accidentally useful effect, or to the whole Morganist scheme in general. The Morganists are trying to appropriate the achievements of our plant breeders—your achievements, Academician Konstantinov, owing to the fact that you did not in time arm yourself with animosity for this false science of Morganism. But you will, I know, Academician Konstantinov, I have faith in you, you are a real plant breeder. (*Prolonged applause.*)

Unfortunately, the corrupting influence of Morganism has penetrated into non-biological circles. Morganism is having a pernicious influence also on some of our philosophers, whose duty it is to have a correct point of view regarding the ideological significance inherent in questions of biology (*applause*), if even Academician Nemchinov, who is not a geneticist but a statistician, if even he has his point of view on the subject of Morganism. (*Laughter and applause.*)

V. S. Nemchinov. Why shouldn't I have my point of view?

I. I. Prezent. I say it not in reproach, but in praise that you have your point of view, although I say it in reproach that you have such a point of view. (*Laughter.*) And so, it is time to put an end to the corrupting influence of the Morganists on workers in other specialities, notably on philosophers. It is the bounden duty of our philosophers to have their own view, and a correct view, on the question as to who solved the problem of directing hereditary variations: Morgan and Muller, or Michurin and Lysenko. Many of our philosophers have been all the time fluctuating on these questions, but, after all, there must be a certain limit to fluctuation. You can't be a pendulum in science. (*Laughter.*) The time has long come for the philosophers of our country to disclose the philosophical depths of the Michurinian theory (*applause*), and I have faith in our philosophers—they will do it.

Comrades, some of the Morganists who spoke here, Academician B. M. Zavadovsky, for example, vowed that they too are Michurinists. As one of the arguments to show that he, too, is a Michurinist, and that he only wants to unite, to reconcile something with something else, Zavadovsky said: "Find me another museum where Michurin is so broadly demonstrated as in the one of which I am director."

If one turns to the utterances of another "semi-Morganist" who spoke from this platform, I. M. Polyakov, one cannot help remarking the astonishing poverty and vacuity of his speech. I can only explain this by the fact that you, Professor Polyakov, desiring at all cost to uphold Morganism and at the same time to pass as a Marxist, have proved a bankrupt; and bankrupts, as we know, have nothing to offer.

But I shall nevertheless dwell on one of I. M. Polyakov's arguments. He said that he cannot dissociate the Michurinists from the Morganists, because the latter also act upon the organism with various substances, X-ray and so forth. But there is action and "action." There is action which operates through the process of development of the organism, takes account of its history, both phylogenetic and individual; and there is action of the type of an abrupt blow, which disregards the biological peculiarities and history of the organism. This blow may be softened, but if it does not operate through the regime of life, through the development, the effect of it can only be accidental. This is not the general road for planned breeding.

I. M. Polyakov, speaking here of wherein he differed from T. D. Lysenko, said that it was not clear to him how Lysenko settled the problem of purposiveness. But in all the works of Lysenko and other Michurinists a clear line of demarcation is drawn between the organism's requirements in definite conditions and the fitness of the effect of meeting these requirements. They point out that selection, and selection alone, creates the purposiveness of the nascent requirement, only selection decides whether it will go down from generation to generation, whether it will accumulate and become a character of the variety or not. And this precisely is the line of demarcation between the erroneous features of Lamarckism and the correct views of T. D. Lysenko and those who think with him. You, Professor Polyakov, do not understand another thing, namely, that the modern Michurinian theory cannot confine itself to the so-called classical Darwinism. The point is not only to purge Darwinism of its sins, of its errors, but also to elevate Darwinism, to raise it to a new level, new in principle, the level of the Michurinian doctrine. Darwinism today is not what it was at the time of Darwin. The law of selection is formulated in the light of the Michurinian doctrine not in the

way it was formulated by Darwin himself. This law of selection necessarily includes the educative role of the conditions, and when we come to speak of artificial selection, that, on the basis of Michurinism, is understood to mean systematic educative selection. This level of selection was unknown to the Darwinian theory; it did not know it and could not know it, but you, Professor Polyakov, are in duty bound to know it. Surely, it is your duty to be wiser than Darwin—if only for the reason that a bird sitting on the head of a sage can see farther than the sage. (*Laughter.*)

Since Morganism is entirely bereft of factual and theoretical arguments, it having been utterly smashed by the facts and arguments which the Michurinists are constantly multiplying, the anti-Michurinists are left with only one tactic, which was characterized in his day by Timiryazev in reference to the infuriated but impotent anti-Darwinists. Timiryazev's words are quite up to date in reference to the anti-Michurinists of today, to whom we may with every justification readdress them.

Different beings resort to different means of defence: the lion defends itself with its claws and the bull with its horns, the hare relies upon the swiftness of its legs, the mouse hides in a hole, and the cuttlefish—it secretes a dark fluid and escapes from its enemy into the murk. Our anti-Darwinists are fond of imitating the tactics of the cuttlefish, with the only difference that the latter is of course glad if it can only get away from its enemy, but our Morganists hurl abuse at their opponent from out of their murky cloud, and complacently cry: "We've smashed him! We've won! He's crushed!"

It is precisely to these cuttlefish tactics that Rapoport, for one, resorted at this session. He declared that the basic concept of the Michurinian genetics—the demand of the organism for definite conditions of life—was nothing more or less than Machism. Rapoport might equally have proclaimed all Darwinism Machism, since the fundamental concept of Darwinism is adaptation, and Mach, as we know, built his false philosophy of empirio-criticism on the absolutely unjustified application of this concept to the processes of cognition.

In addition to the cuttlefish tactics, the Morganists, for lack of scientific arguments, resort to the tactics of organized discrimination against the Michurinists, whenever Morganists

succeed (and that, unfortunately, has happened none too rarely) in securing the helm of an academy, faculty or similar body. Professor Beletsky has spoken of this in reference to the Moscow University. The same thing happened, and even perhaps in more impermissible form, at our University in Leningrad. The dean of the faculty, Lobashev, the prorector, Polyansky, lecturers Airapetiants and Novikov and a group of their like thinkers tried by fair means and foul, more often foul than fair, to drive the Michurinists from the faculty. All sorts of devices were resorted to. It was declared, for instance, that Prezent was not coping with his work, that his lectures were bad. (*Laughter.*) I naturally requested the Ministry to verify these insinuations and stated that if they were found to be true I would quit my post—in other words, would do what Academician Nemchinov promised here to do. (*Laughter and applause.*) In the end, the group which held the key positions were compelled to precisely formulate the motives for their more than persistent desire to remove Prezent from the faculty. They said that Prezent underrated so important a theory as the Mendelian theory, that he underrated the services to Darwinism of Academician Schmalhausen, that he expounded Darwinism critically, that he failed to understand that in our country, where there are no class antagonisms, there cannot be an acute ideological struggle—and from all this the indefeasible conclusion was drawn and a decision was taken stating that it was impossible to retain Prezent in the chair of Darwinism. How are we to regard this?

A voice. Shame, a faction!

I. I. Prezent. What is this, if not organized discrimination?

The attitude of the group that controls the biological faculty at the Leningrad University was recently expressed by Professor Polyansky. Professor Polyansky has just returned from the International Congress in Paris, where he delivered a paper on heredity in protozoa. At present, I think, he is, like Academician B. M. Zavadovsky, on his way to a sanatorium, and for that reason, presumably, could not speak here personally. I shall fill the gap by reading a few passages from a recent address he gave:

"In recent years, T. D. Lysenko has undoubtedly in a number of his works professed profoundly erroneous, harm-

ful and anti-Darwinian views. And this must be said frankly, bluntly and distinctly. I think that by speaking of this frankly we can only be of benefit to T. D. Lysenko, of far greater benefit than if we sing hallelujas and dithyrambs to him, as I. I. Prezent and a number of other comrades do. Erroneous and profoundly harmful is T. D. Lysenko's denial of all the laws established in genetics, his denial of all the principles of Mendelism-Morganism.... It must be stated that in our Soviet biology of late a number of views are advanced and upheld which are profoundly inimical to Darwinism, profoundly inimical to the dialectical method in its concrete application to biology. This should not be glossed over.... These errors are being carried farther, and their effects will be felt in practical work. If the crude Lamarckian standpoint is adopted, plant breeding will be directed along false lines, and this will do the greatest harm to our socialist economy. We must not shut our eyes and must not speak in whispers.

"A struggle is now going on in our biological science, a struggle which will probably soon come to an end, because the unsoundness of these mechano-Lamarckian views is clear to many biologists. The struggle is being waged, on the one hand, from the Marxist standpoint, and, on the other, from the mechano-Lamarckian standpoint, a false standpoint which undoubtedly leads to mechanistic conceptions and to an idealist concept of the evolution of forms." (Y. I. Polyansky, address at the Pokrovsky Pedagogical Institute, May 7, 1948.)

I agree with Professor Polyansky that a struggle really is going on in biological science, and that that struggle probably will come to an end soon. However, I am profoundly convinced that it will not end in the way Polyansky and those who think like him desire, not in the way he dreamed of.

Comrades, we may record with pleasure that our Soviet biologists, armed with the Michurinian doctrine, have already demonstrated the fallaciousness of Morganism up to the hilt. Nobody will be led astray by the Morganists' false analogies between the invisible atom and the invisible gene. Far closer would be an analogy between the invisible gene and the invisible spirit. They want a discussion. But we shall not discuss with the Morganists (*applause*); we shall continue to expose them as adherents of an essentially false scientific trend, a

pernicious and ideologically alien trend, brought to our country from foreign shores. (*Applause*). We, Michurinists, will argue and discuss Michurinian problems within the framework of the Michurinian doctrine, and thereby develop this remarkable doctrine by the efforts of the entire body of the many thousands of Michurinists. We look boldly forward to the future. The Michurinists have no need to promise that they are on the threshold of big discoveries. As a body, the Michurinists have already made big discoveries, discoveries of world-wide significance.

We look boldly forward to the future because we have a real leader, while you, the Morganists, have—Schmalhausen. (*Loud and prolonged applause.*) The Morganists are endeavouring to hold back the Michurinian doctrine by contrasting Michurin to Lysenko, the earlier Lysenko to the later Lysenko, and Lysenko to his followers. That is what retrogrades may be expected to do. They know that every new advance spells their ruin. The Morganists want to stop the onward march of the Michurin movement. But in vain. They have not succeeded, are not succeeding, and will not succeed.

The future in biology belongs to Michurin, and only to Michurin. And with this, permit me to conclude. (*Applause.*)

Academician P. P. Lobanov. The session is adjourned until August 7, 11 a.m.

TENTH SITTING

Morning, August 7, 1948

Academician P. P. Lobanov. The session is resumed. Motions have been received calling for the termination of the debate. In all, 72 persons have asked for the floor, of whom 56 have spoken. In addition, eleven persons have asked for the floor a second time. What is the pleasure of the session?

Voices. Close the debate.

Academician P. P. Lobanov. I shall put the motion for the closure to the vote. All those in favour, raise your hands. Thank you. Those against? (*None.*)

Adopted unanimously.

Academician Trofim Denisovich Lysenko, President of the Lenin Academy of Agricultural Sciences of the U.S.S.R., now has the floor for his concluding remarks. (*Loud and continuous applause.*)

CONCLUDING REMARKS
BY ACADEMICIAN T. D. LYSENKO

Comrades, before I pass to my concluding remarks I consider it my duty to make the following statement.

The question is asked in one of the notes handed to me, What is the attitude of the Central Committee of the Party to my report? I answer: The Central Committee of the Party examined my report and approved it. (*Stormy applause. Ovation. All rise.*)

I shall now take up some of the points brought out at our session.

The adherents of the so-called chromosome theory of heredity who spoke here denied that they were Weismannists and all but proclaimed themselves antagonists of Weismann. On the other hand, it has been clearly shown in my report and in many of the speeches of representatives of the Michurin trend that Weismannism and the chromosome theory of heredity are one and the same thing. Mendelist-Morganists abroad make no secret of this. In my report I quoted articles by Morgan and Castle published in 1945, in which it is plainly stated that the so-called teaching of Weismann is the basis of the chromosome theory of heredity. By Weismannism (which is the same as idealism in biology) is meant any conception of heredity which maintains that the living body is divided into two substances which are different in principle: the ordinary living body, presumably possessing no heredity but subject to alterations and transformations, that is to say, to development; and a specific hereditary substance, presumably independent of the living body and not subject to development under the influence of the conditions of life of the ordinary living body, or the *soma*. That much is beyond any doubt. No efforts of the advocates of the chromosome theory of heredity, neither those who spoke nor those who did not speak at the session, to lend their theory a materialist appearance can change the character of this theory, which is essentially idealistic. (*Applause.*)

The Michurin trend in biology is a materialist trend, because it does not separate heredity from the living body and the conditions of its life. There is no living body without heredity, and there is no heredity without a living body. The living body and its conditions of life are inseparable. Deprive an organism of its conditions of life and the living body will die. The Morganists, however, maintain that heredity is isolated, something apart from the mortal living body, from what they call the soma.

Those are the principles on which we differ with the Weismannists. And connected with them is also our difference on a question which has a long history behind it, namely, the question of inheritance of characters acquired by plants and animals. The Michurinists say that inheritance of acquired characters is possible and necessary. This principle has once more been fully confirmed by the abundant factual material demonstrated at this session. Morganists, among them those who spoke at our session, cannot comprehend this principle so long as they have not fully discarded their Weismannist notions.

It is still not clear to some that heredity is inherent not only in the chromosomes, but in any particle of the living body. They therefore want to see with their own eyes cases of hereditary properties and characters transmitted from generation to generation without the transmission of chromosomes.

These questions, so incomprehensible to the Morganists, can best be answered by demonstrating and explaining experiments in vegetative hybridization carried on extensively in our country. It was I. V. Michurin who elaborated vegetative hybridization. And experiments in vegetative hybridization show incontrovertibly that heredity is a property not only of the chromosomes, but of every living thing, every cell and every particle of the body. For heredity is determined by the specific type of metabolism. You need but change the type of metabolism in a living body to bring about a change in heredity.

Academician P. M. Zhukovsky, as becomes a Mendelist-Morganist, cannot conceive transmission of hereditary properties without transmission of chromosomes. He cannot con-

ceive that the ordinary living body possesses heredity. According to his views, that is the property of the chromosomes only. He therefore does not think it possible to obtain plant hybrids by means of grafting, he does not think it possible for plants and animals to inherit acquired characters. I promised Academician Zhukovsky to show him vegetative hybrids, and I have now the pleasure of demonstrating them at this session.

In this case one of the participating plants was a variety of tomatoes with leaves not dissected, as usual, but like those of the potato. Its fruits are red and oblong in shape.

The other variety that participated in the grafting was one with the usual dissected tomato leaves. The fruits when ripe are not red, but yellowish, white.

The variety with the potato leaves was used as the stock, and the variety with the dissected leaves was the scion.

In the year when the graft was made no changes were observed either in the scion or in the stock.

Seeds were gathered from the fruits that had grown on the scion and from those that had grown on the stock. These seeds were then planted.

Most of the plants that grew from the seeds taken from the fruits of the stock did not differ from the initial variety, that is to say, they were potato-leafed and their fruits were red and oblong in shape. Six plants, however, had dissected leaves, and some of them had yellow fruits, that is to say, both the leaves and the fruits had changed under the influence of the other variety, the one which had been the scion.

Academician P. M. Zhukovsky has expressed doubt as to the purity of the experiments in vegetative hybridization, pointing out that cross-pollination of the varieties might have occurred—in other words, that it was a case of sexual hybridization. But how, Comrade Zhukovsky, can the results of the experiments I demonstrate be explained by cross-pollination?

All who have had anything to do with the hybridization of tomatoes know that when the plants with dissected leaves and yellow fruits are cross-pollinated with the plants with potato leaves and red fruits, the first generation will have dissected leaves, but invariably red fruits.

But see what we have got in our experiments. The leaves are indeed dissected, but the fruits are not red but yellow. How,

then, can these results be explained by accidental cross-pollination?

Here are the fruits of the other of these vegetative hybrids. The leaves of this plant are also dissected, but of the ripe fruits on the cluster, one, as you see, is red and the other yellow. Variety within a single plant is a quite frequent phenomenon among vegetative hybrids. It should be borne in mind that vegetative hybridization is not the usual mode of union of breeds, not the one that has developed in the course of their evolution. That is why as the result of grafting we often get organisms that are destabilized and therefore prone to vary.

It is not in all plants by any means that we can observe easily perceptible alterations in the year of the grafting or even in the first seed generation. None the less we already have every ground to assert that every graft of a phasically young plant produces changes in heredity. To prove this point we are going on with our work on vegetative hybrids of tomatoes at the Institute of Genetics of the Academy of Sciences of the U.S.S.R.

I shall now show you plants of the second seed generation obtained from the same graft; but these are from seeds taken from plants which gave no visible alterations in the first seed generation. On a number of plants from the second seed generation the leaves are changed—they are not like potato leaves in appearance, but dissected, and the fruits are not red but yellow. In this case, too, there is no reason to doubt the purity of the work or to suspect cross-pollination. In the first generation these plants had potato leaves and red fruits. If the dissected leaves in the plants of the second generation are the result of cross-pollination, why are the fruits not red but yellow?

We thus see that as the result of grafts we obtain directed, adequate alterations; we obtain plants combining the characters of the breeds joined in the grafting, that is to say, we get true hybrids. New formations are also observed. For example, among the progeny of the same graft there are plants that have borne small fruits, like those of uncultivated forms. But we all know that in the case of sexual hybridization, too, we observe, besides the transmission to the progeny of characters of the parent forms, also the appearance of new forms.

I could cite many more cases of the production of vegeta-

tive hybrids. It is no exaggeration to say that there are hundreds and thousands of them in our country. The Michurinists not only understand how vegetative hybrids are produced, but produce them in large numbers from numerous varieties.

I have dwelt at length on vegetative hybrids because they provide instructive material of great significance. For not only Mendelists, but even materialists who have not seen vegetative hybrids, may refuse to believe that everything that is alive, every particle of a living body, possesses heredity as well as the chromosomes. This can be easily demonstrated by the examples of vegetative hybridization. Chromosomes cannot pass from stock to scion and vice versa—that is a fact no one disputes. Yet hereditary properties, such as the colouring of the fruit, its shape, the shape of the leaves, and others, are transmitted from scion to stock and from stock to scion. Now show us any properties of two breeds united into one by means of sexual hybridization—in the case of tomatoes, for instance—which could not be united or have not been united by the Michurinists, by means of vegetative hybridization.

Thus experiments in vegetative hybridization provide unmistakable proof that any particle of a living body, even the plastic substances, even the sap exchanged between scion and stock, possesses hereditary qualities.

Does this detract from the role of the chromosomes? Not in the least. Is heredity transmitted through the chromosomes in the sexual process? Of course it is.

We recognize the chromosomes. We do not deny their existence. But we do not recognize the chromosome *theory* of heredity. We do not recognize Mendelism-Morganism.

Let me remind you that Academician P. M. Zhukovsky promised that if I showed him vegetative hybrids, he would believe and revise his position. I have now kept my promise and shown him vegetative hybrids. But I must remark, firstly, that dozens and hundreds of such hybrids could be seen in our country for at least a decade now; and, secondly, is it possible that Academician Zhukovsky, a botanist, does not know what is known to many, even if not all, horticulturists—namely, that in decorative horticulture a great deal has been done, and is being done, to change the heredity of plants by means of grafting?

Some of the Morganists who spoke at this session alleged that, together with the chromosome theory of heredity, Lysenko and his followers reject all the experimental facts obtained by Mendelist-Morganist science. Such allegations are false. We do not reject any experimental facts, and this holds good for the facts concerning chromosomes.

Some go so far as to assert that the Michurin trend denies the action upon plants of the so-called mutagenic factors, such as X-rays, colchicine, etc. But how is it possible to assert anything of the sort? Certainly, we Michurinists cannot deny the *action* of such factors. We recognize the action of the conditions of life upon the living body. Why then should we refuse to recognize the action of such potent factors as X-rays or a strong poison like colchicine, etc.? We do not deny the action of the so-called mutagenic substances. But we insist that such action, which penetrates into the organism not in the course of its development, not through the process of assimilation and dissimilation, can only rarely and only *fortuitously* lead to results useful for agriculture. It is not the road of systematic selection, not the road of progressive science.

The protracted and numerous efforts made in the Soviet Union to produce polyploid plants with the aid of colchicine and similar potent factors have in no way led to the results so widely advertized by the Morganists.

A great deal has been said and written to the effect that a geranium began to give seeds after its chromosome complement had been increased. But this geranium is not being grown for the market, and I, as a scientist, venture the opinion that it never will be so grown, because it is much more practical to propagate geraniums by cuttings. Currants, for example, can be grown from seeds, but in practice they are propagated by cuttings. Potatoes can also be grown from seeds, but it is more practical to plant tubers. As a rule, plants which can be propagated both by seeds and by cuttings (i. e., by the vegetative method) are propagated for practical ends by the latter method.

This does not mean that we minimize the importance of the fact that a geranium has been obtained which is capable of producing seeds. If not for practical ends, this form can be of use in the study of plant breeding.

And what I have said of geraniums applies also to mint.

What other polyploids are often represented by the Morganists as highly important achievements? Wheat, millet, buckwheat, and a few other plants. But, according to the statements which we have heard here from the Morganists themselves (A. R. Zhebrak, for example), all these polyploids—wheat, millet, buckwheat—have so far, as a rule, been found to be of small fertility, and their authors themselves have refrained from recommending their cultivation for practical ends.

There only remains the tetraploid kok-saghyz. This is the first year it is being tested on collective farms. It goes without saying that, if it proves to be good, it ought to be introduced in practical farming. So far, however, according to the data of three years of government seed testing, it is not superior to the ordinary diploid varieties, such as Bulgakov's, for example. This is the first year tetraploid kok-saghyz is being tested on collective farms. In another two or three years we shall have practical proof of how good it is. I sincerely wish that it may prove to be the best of all kok-saghyz forms. Our agriculture can only gain thereby.

At the same time we must not forget that among the varieties of cultivated plants there are plenty of polyploids whose origin has nothing to do with colchicine and the mutagenic theory, nor, for that matter, with the theory of Morganism-Mendelism as a whole. For centuries people did not know that many good varieties of pears, for example, are polyploids. But we have also as many equally good varieties of pears which are not polyploids. These facts alone provide enough grounds for the conclusion that it is not the number of chromosomes that determines the quality of a variety.

There are good and bad varieties of durum wheat with 28 chromosomes, and there are good and bad varieties of soft 42-chromosome wheat.

Is it not obvious that breeding must be conducted, not with a view to the number of chromosomes, not with a view to polyploidy, but with a view to inducing good qualities and properties?

When a good variety has been produced, we can also determine the number of its chromosomes. But no one, certainly,

will think of discarding a good variety only because it has turned out to be a polyploid or not a polyploid. No Michurinist, no serious-minded person generally, can approach the question from such an angle.

Our Morganists, among them some who spoke at this session, in order to adduce proof that their theory is effective, often point to some varieties of cereal grains which are widespread in practical farming, as, for example, Lutescens 062, Melanopus 069, and some other varieties of long standing which they claim have been produced on the basis of Morganism-Mendelism. But actually Mendelism has nothing to do with the production of these varieties. How, for example, have varieties like Lutescens 062, Melanopus 069, Ukrainka, and some others been produced? They were produced by the ancient method of selection from local varieties.

I shall quote here Professor S. I. Zhegalov, who wrote in his work, *An Introduction to the Selection of Agricultural Plants*: "Under ordinary farming conditions we have to deal, not with pure forms, but with 'varieties' representing more or less complex combinations of various forms. . . . The first, perhaps, to draw attention to this fact in the first quarter of the nineteenth century [long before the appearance of Weismannism—*T. L.*] was the Spanish botanist Mariano Lagasca, who published his observations in Spanish. There is an interesting story extant about a visit he paid to his friend, Colonel Le Couteur, at the latter's estate on Jersey Island. During an inspection of the fields he drew the attention of his host to the considerable diversity of forms among the plants and suggested that individual forms be selected for further pure breeding. The idea appealed to Le Couteur, who selected twenty-three different forms and began to test their relative merits. As a result of the tests, he found one of the forms to be the very best, and in 1830 put it on the market as a new variety named 'Talavera de Bellevue.' Since then this kind of work has been tried many times, and it has led to the production of many valuable varieties. In substance, it consists in separating the initial mixtures into their component parts. That is why this method is known as '*analytical selection.*' At present it is the principal method employed in work with self-pollinating plants and is systematically applied by all stations, particu-

larly in the early stages of the work on plants formerly little affected by selection."[1]

A little farther Professor S. I. Zhegalov writes: "The method of analytical selection lends meaning to an aphorism credited to Jordan: 'In order to produce a new variety, we must first have it.' "[2]

Comrade Shekhurdin, was the form of wheat now called Lutescens 062 to be found among the local Poltavka variety or not? [*Voice from the audience*: "Yes, positively."] The same is true of the forms called Ukrainka and Melanopus 069.

That is why S. I. Zhegalov accepts the aphorism that in applying the method of analytical selection it is necessary, in order to produce a new variety, first to have it. The named varieties, to which our Mendelists usually point, have indeed been obtained in this manner.

We Michurinists, however, cannot agree with Professor S. I. Zhegalov and his interpretation of Darwinian selection. For it is possible to begin to select plants with scarcely perceptible, still feeble useful characters, in order to reinforce and develop these useful characters by repeated selection and proper cultivation. But, as is obvious to any one, the described Darwinian method of selection has no bearing whatever on the Mendel-Morgan theories.

It should be mentioned that formerly varieties were bred only on the basis of the above method. For that matter, this method is being applied today and will be applied in future. It is a useful method, and practical breeders who successfully apply it should be appreciated and encouraged.

Far from rejecting the method of continuous improving selection, we, as is well known, have always insisted on it. The Morganists, on the other hand, have ridiculed the application of repeated improving selections in practical seed growing.

Weismannism-Morganism has never been, nor can it be, a science conducive to the systematic production of new forms of plants and animals.

It is significant that abroad, in the United States for example, which is the home of Morganism and where it is so highly ex-

[1] С. И. Жегалов, *Введение в селекцию сельскохозяйственных растений*, 1930 г., стр. 79-80.

[2] *Ibid.*, p. 83.

tolled as a theory, this teaching, because of its inadequacy, has no room in practical farming. Morganism as a theory is being developed *per se,* while practical farmers go their own way.

Weismannism-Morganism does not reveal the real laws of living nature; on the contrary, since it is a thoroughly idealistic teaching, it creates an utterly false idea about natural laws.

For instance, the Weismannist conception that the hereditary characteristics of an organism are independent of environmental conditions has led scientists to affirm that the property of heredity (i. e., the specific nature of an organism) is subject only to chance. All the so-called laws of Mendelism-Morganism are *based entirely on the idea of chance.*

Here are a few examples.

"Gene" mutations, according to the theory of Mendelism-Morganism, appear fortuitously. Chromosome mutations are also fortuitous. Due to this, the direction of the process of mutation is also fortuitous. Proceeding from these invented fortuities, the Morganists base their experiments too on a fortuitous choice of substances that might act as mutagenic factors, believing that they are thereby acting on their postulated hereditary substance, which is just a figment of their imagination, and hoping to obtain fortuitously what may by chance prove to be of use.

According to Morganism, the separation of the so-called maternal and paternal chromosomes at reduction division is also a matter of pure chance. Fertilization, according to Morganism, does not occur selectively, but by the chance meeting of germ cells. Hence the segregation of characters in the hybrid progeny is also a matter of chance, etc.

According to this sort of "science" the development of an organism does not proceed on the basis of the selection of conditions of life from the environment, but again on the basis of the assimilation of substances fortuitously entering from without.

In general, living nature appears to the Morganists as a medley of fortuitous, isolated phenomena, without any necessary connections and subject to no laws. Chance reigns supreme.

Unable to reveal the laws of living nature, the Morganists have to resort to the theory of probabilities, and, since they fail to grasp the concrete content of biological processes, they

reduce biological science to mere statistics. It is not for nothing that statisticians like Galton, Pearson, and latterly Fisher and Wright, are also regarded as founders of Mendelism-Morganism. Probably that is also the reason why Academician Nemchinov has told us here that, as a statistician, he found that he could easily take in the chromosome theory of heredity. (*Amusement, applause.*)

Mendelism-Morganism is built entirely on chance; this "science" therefore denies the existence of necessary relationships in living nature and condemns practical workers to fruitless waiting. There is no effectiveness in such science. With such a science it is impossible to plan, to work toward a definite goal; it rules out scientific prediction.

A science which fails to give practical workers a clear perspective, the power of finding their bearings and confidence that they can achieve practical aims does not deserve to be called science. (*Applause.*)

Physics and chemistry have rid themselves of fortuities. That is why they have become exact sciences.

Living nature has been developing and is developing on the basis of strict laws inherent in it. Organisms and species develop in line with natural necessities inherent in them.

By ridding our science of Mendelism-Morganism-Weismannism we will expel fortuities from biological science. (*Applause.*)

We must firmly remember that *science is the enemy of chance.* (*Loud applause.*) That is why Michurin, who was a transformer of nature, put forward the slogan: "We cannot wait for favours [i. e., lucky chance—*T. L.*] from Nature; we must wrest them from her." (*Applause.*)

Aware of the practical sterility of their theory, the Morganists do not even believe in the possibility of the existence of an effective biological theory. Ignorant even of the ABC of the Michurinist science, they cannot to this day imagine that for the first time in the history of biology a truly effective theory has come into being—the Michurin teaching. (*Applause.*)

A great deal can be scientifically predicted on the basis of the Michurin teaching, thus freeing practical plant breeders to an ever-increasing extent from the elements of chance in their work.

Michurin himself elaborated his theory, his teaching, only in the process of solving problems of practical importance, in the process of the production of good varieties. That is why *the Michurin teaching is, by its very spirit, inseparable from practice.* (*Applause.*)

Our kolkhoz system, our socialist agriculture, created all the conditions for the flowering of the Michurin teaching. Let us recall Michurin's words: "In the person of the collective farmer the history of agriculture of all times and all nations has an entirely new type of farmer, one who has taken up the struggle with the elements marvellously armed technically and acting on nature as a man with the views of a renovator."[1]

"I see," wrote I. V. Michurin, "that the system of collective farming, by means of which the Communist Party is inaugurating the great work of renovating the land, will lead labouring humanity to real dominance over the forces of nature.

"The great future of our entire natural science is in the collective farms and state farms."[2]

The Michurin teaching is inseparable from the practical collective farm and state farm activity. It is the best form of unity of theory and practice in agricultural science.

It is clear to us that the Michurin movement could not develop extensively, if there were no collective farms and state farms.

Without the Soviet system, I. V. Michurin would have been, as he himself wrote, "an obscure hermit of experimental horticulture in tsarist Russia."[3]

The strength of the Michurin teaching lies in its close association with the collective farms and state farms, in the fact that it *elucidates profound theoretical problems by solving important practical problems of socialist agriculture.*

Comrades, our session is drawing to its close. This session has vividly demonstrated the strength and potency of the Michurin teaching. Many hundreds of representatives of biological and agricultural science have taken part in it.

They have come here from all parts of our vast country.

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[1] И. В. Мичурин, *Сочинения,* том I, стр. 477.
[2] *Ibid.*
[3] *Ibid.*, Vol. IV, p. 116.

They have taken an active part in the discussion on the situation in biological science and, convinced in the course of many years of practical activity that the Michurin teaching is right, are ardently supporting this trend in biological science.

The present session has demonstrated *the complete triumph of the Michurin trend over Morganism-Mendelism.* (*Applause.*)

It is truly a historic landmark in the development of biological science. (*Applause.*)

I think I shall not be wrong if I say that this session has been a great occasion for all workers in the sciences of biology and agriculture. (*Applause.*)

The Party and the Government are showing paternal concern for the strengthening and development of the Michurin trend in our science, for the removal of all obstacles to its further progress. This imposes upon us the duty to work still more extensively and profoundly to arm the state farms and collective farms with an advanced scientific theory. That is what the Soviet people expect of us.

We must effectively place science, theory, at the service of the people, so that crop yields and the productivity of stock-breeding may increase at a still more rapid pace, that labour on state farms and collective farms may be more efficient.

I call upon all Academicians, scientific workers, agronomists, and animal breeders to bend all their efforts and work in close unity with the foremost men and women in socialist farming to achieve these great and noble aims. (*Applause.*)

Progressive biological science owes it to the geniuses of mankind, *Lenin* and *Stalin,* that *the teaching of I. V. Michurin has been added to the treasure house of our knowledge, has become part of the gold fund of our science.* (*Applause.*)

Long live the Michurin teaching, which shows how to transform living nature for the benefit of the Soviet people! (*Applause.*)

Long live the Party of Lenin and Stalin, which discovered Michurin for the world (*applause*) and created all the conditions for the progress of advanced materialist biology in our country. (*Applause.*)

Glory to the great friend and protagonist of science, our leader and teacher, Comrade Stalin! (*All rise. Prolonged applause.*)

Academician P. P. Lobanov. Academician P. M. Zhukovsky desires to make a statement.

Academician P. M. Zhukovsky. Comrades, late yesterday evening I decided to make this statement. I say late yesterday evening deliberately, because I did not know then of the letter of Yuri Zhdanov which appeared in *Pravda* today. There is therefore no connection between my present statement and Yuri Zhdanov's letter. I think Vice Minister of Agriculture Lobanov will bear this out, since I phoned him yesterday evening and requested permission to make a statement at today's meeting of the session.

There are moments in a man's life, especially in our historic days, which are to him of profound and crucial moral and political significance. This is what I experienced yesterday and today. The speech I made the day before yesterday was an unhappy one; it was the last of my speeches against Michurin, as it is said here to have been, although I personally have never before spoken in opposition to Michurin's teachings. At the same time, it was my last speech from an incorrect biological and ideological standpoint. (*Applause.*)

My ill-starred article, "Darwinism in a Crooked Mirror," and our President's reply to that article led me to abandon the field of ideological controversy and to nurse a sense of personal injury. True, I still continue to hold the view that intraspecific competition does exist. But what I want to say is that it was precisely in this period that my personal attitude towards the President was very seriously strained.

The speech I made the day before yesterday, at a time when the Central Committee of the Party had drawn a dividing line between the two trends in biological science, was unworthy of a member of the Communist Party and of a Soviet scientist.

I admit that the position I held was wrong. Academician Lobanov's noteworthy speech yesterday, and I esteem P. P. Lobanov as a fine statesman, his words directly addressed to me—our ways must part—moved me deeply. His speech agitated me profoundly. A sleepless night helped me to think over my behaviour.

Academician Vasilenko's speech also made a deep impres-

sion upon me, for he showed how closely the Michurinists are connected with the people, and how important it is at this juncture to cherish the prestige of our President.

The exceptional unity displayed by the members and guests at this session, the demonstration of the power of this unity and the bonds with the people, and, on the contrary, the demonstration of the weakness of the opponents are to me so obvious, that I declare that I shall fight—and there are times when I can fight—for the Michurinian biological science. (*Prolonged applause.*)

I am a man of responsibility, for I am a member of the Stalin Prize Committee of the Council of Ministers and a member of the Committee of Experts on the award of high scientific degrees. I therefore consider that it is my moral duty to be a sincere Michurinist, a sincere Soviet biologist.

Comrades Michurinists, when I declare that I pass over to the ranks of the Michurinists and will defend them, I do so honestly. I turn to the Michurinists, among whom I have both friends and opponents, and say that I will honestly carry out what I proclaim here today. (*Applause.*)

I am sure that, knowing me, you will believe me when I say that it is not from cowardice that I make this statement. One of the distinguishing features of my character, all through life, has been a deep impressibility. Everyone knows how sensitively I react to everything. You can therefore believe me when I say that this session has indeed made a deep impression upon me.

It has been said here (and the reproach is deserved) that we do not conduct a fight in the press against foreign reactionaries in the field of biological science. I declare here that I shall conduct that fight, that I attach political importance to it. I consider the time has come when the voice of Soviet biologists must at last be heard in our scientific press on the subject of the deep ideological abyss that divides us. And only those foreign scientists who realize that the bridge must be thrown from them to us, and not from us to them, can hope to have our attention.

Let the past which divided me from T. D. Lysenko (although not always, it is true) be forgotten. Believe me, that I

take this step today as a Party member, as a sincere member of our Party—that is, honestly. (*Applause.*)

At the same time, I declare that Academician Vasilenko's appeal to cherish the prestige of our President will be observed by me. (*Applause.*)

Academician P. P. Lobanov. Comrade S. I. Alikhanian wishes to make a statement.

S. I. Alikhanian. Comrades, it was not because I had read Yuri Andreyevich Zhdanov's statement in today's *Pravda* that I requested the chairman to allow me the floor. I decided yesterday to make a statement, and Vice Minister of Agriculture P. P. Lobanov can confirm that I spoke to him on the subject yesterday, August 6.

I have followed this session very attentively, and I have lived through a lot in these days. From all that has taken place at this session, from all that I, as a scientist, have pondered over, it behoves me, as a young Soviet scientist, to draw a fundamental conclusion. What is involved, comrades—I here address my like-thinkers....

N. G. Belenky. Past or present?

S. I. Alikhanian. Both past and present. The issue involved is a struggle between two worlds, between two world outlooks, and it would be senseless to cling to the old tenets which were inculcated in us by our teachers.

We strongly succumbed to the polemical passion kindled by our teachers in this controversy. Because of this polemical ardour we were unable to discern the new and growing trend in genetic science—the Michurinian trend. And, as I have said, it is important to realize that we must be on this side of the scientific barricades, with our Party and with our Soviet science.

It would be foolish to think that we are being asked to discard everything good and useful accumulated in the course of the development of science. What we are being asked is to discard everything reactionary, false and useless. And we must do so sincerely and honestly, as befits real scientists.

I appeal to my comrades to draw very serious conclusions from these words of mine. I, as a Communist, cannot and

must not, in the ardour of controversy, obstinately oppose my personal views and concepts to the onward march of biological science.

When I leave this session, the first thing I must do is to review not only my attitude towards the new, Michurinian science, but my entire earlier activity. I call upon my comrades to do likewise.

I cannot conceive existence without useful and active work for the benefit of Soviet society and of Soviet science. I believed in our Party, in our ideology, when I went into battle with my soldiers in the war. And today I sincerely believe that, as a scientist, I am acting honestly and truthfully, and marching in step with my Party, with my country. And, comrades, if you do not do likewise, you will find yourselves trailing in the rear, lagging behind the progressive development of science. Science does not tolerate irresolution and unprincipled wavering.

From tomorrow on I shall not only myself, in all my scientific activity, try to emancipate myself from the old reactionary Weismann-Morganian views, but shall try to reform and convince all my pupils and comrades.

There is no denying that this will be an extremely difficult and painful process. Many perhaps will not understand this; but then there is nothing to be done—our way and their way will part. It will mean that they cannot appreciate the assistance the Party has rendered us in this radical turn which has taken place in science, that they cannot understand that it is not a matter of differences on individual questions of no fundamental importance in principle.

I shall strive to have my comrades not waste their experience and knowledge in vain, not leave them in their laboratories, but apply them broadly to the needs of the national economy. This is not hard to do, provided we rid ourselves of the deadweight of useless, metaphysical concepts, and honestly go forward to the end in close fellowship with all the scientists of our country.

And only in our country, the country with the most advanced and progressive world outlook, can the seedlings of the new scientific trend develop. And our place is with this new, progressive trend. I, for my part, categorically declare

to my comrades that I shall henceforth contend with those whose views I used to share and who do not understand this and do not take the way of the Michurinian trend. I shall not only criticize the vicious elements of Weismannism-Morganism which my work contained; I shall take an active part in the onward advance of the Michurinian science.

I am sure that all the biologists at the Moscow University will correctly understand me, and that we shall convert the foremost university of our country—the Moscow State University—into a centre of propagation of the Michurinian doctrine, into a centre of promotion of the Michurinian biology. (*Applause.*)

Academician P. P. Lobanov. Professor I. M. Polyakov has the floor for a statement.

I. M. Polyakov. Comrades, yesterday evening I said in conversation with friends who are present here today that this session was a big event in my life, that it has agitated me deeply and caused me to revalue many of my ideas.

Comrades are familiar with my speeches at numerous scientific congresses and conferences, with my scientific articles and textbooks. I have always striven honestly to analyze and understand the big and important issues in the theory of evolution, in Darwinism and genetics. I have striven to weigh fundamental theoretical principles in our science from the Marxist-Leninist standpoint, to trenchantly criticize the reactionary views of foreign scientists and of some of our scientists. Comrades know that I have done this for many years.

But when the reproach was levelled at me from this platform that my speech was vague, the reproach was justified. Not to go to the end, to take up an intermediate position, is something unworthy of a Bolshevik scientist. One must formulate one's position clearly and distinctly. One must frankly say that the Michurinian trend is the highroad of development of our biological science, and this is the road we must follow. It is the only possible road for Bolsheviks, Party and non-party, who desire to work in the field of our biological science and bring benefit to our Soviet people, to our Country.

I should like to remark that for the past eight or nine years I and my close associates have been working on the problem of elective fertilization, one of the most important problems of the Michurinian genetics. We have arrived at a number of interesting conclusions, and these conclusions fully bear out the Michurinian view. I have written about this in works which are now in the press. But one cannot stop there; further conclusions have to be drawn. One must be logical and not try to reconcile irreconcilables.

For the scientist who takes his science seriously and loves it, to change front in short order is hardly a seemly thing. On many issues I still have to do a great deal of serious thinking. On a number of problems of our science we can and should argue fruitfully and constructively. If, for example, we argue over the struggle for existence and selection, there is nothing amiss in that, since comradely disputes among Soviet scientists on concrete scientific problems can be only useful. But it is necessary to understand the chief and fundamental thing, namely, that our Party has helped us to effect a profound and radical reconstruction of our science, has shown us that the Michurinian theory defines the basic line of development of Soviet biological science, and from this we must draw the conclusion and work to promote the Michurinian trend. And this must be demonstrated by one's work, and not simply by words. This must be the program of my work as a Communist scientist. If you do not take this road then, willy-nilly, you will attract people with a penchant for unprincipled factionalism, people who are unable to see behind individual scientific disputes the big and fundamental things that are being done in our country. I urge all our Soviet biologists to come to the same conclusion that I have come to. For many this will not be simple or easy; it will require deep and serious thinking. But, I repeat, it is necessary completely to break with the false views, vigorously to criticize the metaphysical, idealist, Weismannist views of foreign reactionaries in the field of science and the re-echoings of these views in the works of certain Soviet scientists. We must assist our Party in exposing the reactionary pseudoscientific rot which is disseminated by our enemies abroad. We must realize that this rot has influenced some of our Soviet scien-

tists, and it must be utterly eliminated. The Michurinian trend of science, headed by T. D. Lysenko, is a broad and profoundly scientific popular movement, a movement which helps us to proceed more swiftly along the great road of triumphant building of Communist society. It is in this trend that Soviet biological and agricultural scientists must work. It is in this trend that I too will work, devoting all my strength to the promotion of the great Michurinian theory.

Academician P. P. Lobanov. A motion has been received to send on behalf of the participators at this session a message of greeting to Comrade J. V. Stalin. (*Loud and continuous applause and cheers.*)

The draft of the letter will be read by Academician I. D. Kolesnik.

(*Academician I. D. Kolesnik reads the draft of a message of greeting to J. V. Stalin.*)

From the Session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. to *J. V. STALIN*

Dear Joseph Vissarionovich,

The Academicians, agronomists, animal breeders, biologists, farm mechanization experts and organizers of socialist agriculture present at this session of the Lenin Academy of Agricultural Sciences send you their hearty Bolshevik greetings and best wishes.

Every day and every hour, agricultural scientists and practical workers are conscious of the deep solicitude of the Communist Party and the Soviet Government for agricultural science, and of your constant personal efforts for its promotion and progress.

Science in our country owes a debt of gratitude to you, the great builder of Communism, for having in your works of genius enriched and elevated it in the eyes of the world, for having protected it from the danger of becoming divorced from the needs of the people, for helping it to triumph over reactionary doctrines inimical to the people, and for concerning yourself for the constant widening of the intellectual horizon of our scientific workers.

Continuing the work of V. I. Lenin, you saved for progressive, materialist biology the teachings of that eminent transformer of nature, I. V. Michurin; you held up the Michurin trend in biology to science as the only correct and progressive trend in all branches of biology. This has helped still further to strengthen the natural-scientific foundations of the Marxist-Leninist world outlook, the all-conquering power of which is confirmed by the whole experience of history.

You, our dear leader and teacher, constantly help Soviet scientists to develop our progressive materialist science, which serves the people in their labours and valorous endeavours, the science which reflects the world outlook and noble aims of the citizen of the new, socialist society.

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The collective farm system, created under your wise guidance, has opened up boundless possibilities for a powerful expansion of productive forces in all branches of agriculture and has demonstrated its invincible strength. The Party of Lenin and Stalin has reared among the collective farm peasantry splendid fighters for high yields in agriculture and animal husbandry. The Michurinian agricultural science, urged by you more boldly and determinedly to develop research in the active transformation of the nature of plants and animals, arms the practical farmers in their struggle for a highly efficient socialist agriculture. In their turn, the foremost men and women in the collective farms—the agricultural innovators—stimulated by countrywide socialist competition, are enriching our science with new methods and new achievements.

We assure you, dear Joseph Vissarionovich, that we shall bend every effort to assist the collective farms and state farms in securing even higher yields from our socialist fields and higher productivity of collective farm and state farm animal husbandry, in order to ensure an abundance of produce in our country as one of the major conditions for the transition from Socialism to Communism. We see the possibility of achieving this lofty aim in close unity between science and the people, between science and the foremost men and women in the collective farms, as you are constantly teaching us, Party and non-party Bolsheviks. A science which hedges itself off from the people, from practice, is no science.

Our agrobiological science, developed in the works of Timiryazev, Michurin, Williams and Lysenko, is the foremost agricultural science in the world. It is not only the lawful heir of the progressive ideas of the advanced scientists in human history; it represents a new and higher level of development of human knowledge in the realm of agricultural efficiency. The Michurinian doctrine is a new and higher stage in the development of materialist biology. The Michurinian biological science will continue creatively to develop Darwinism, unswervingly and determinedly to expose the reactionary, idealist Weismann-Morganian scholasticism, which is divorced from practice, to combat the servile worship of bourgeois science which is unworthy of Soviet scientists, and to emancipate researchers from survivals of idealist, metaphysical ideas. Pro-

gressive biological science repudiates and exposes the false idea that it is impossible to govern the nature of organisms by creating man-controlled conditions of life for plants, animals, microorganisms.

Science should teach the experimenter to be audacious in the search for ways and means of governing nature in the interest of man.

To this we are inspired by the teachings of Marx, Engels, Lenin and Stalin, which have proved so triumphant in science and practice.

To this we are enthused by your words about progressive science, science which serves the people, science which values traditions, but does not fear to raise its hand against all that is obsolete.

Hail the progressive Michurinian biological science!

Glory to the great Stalin, the leader of the people and coryphaeus of progressive science!

(*Stormy, prolonged and mounting applause and cheers. All rise.*)

Academician P. P. Lobanov. Permit me to interpret your applause as a unanimous endorsement of the message of greeting to Comrade J. V. Stalin. (*Stormy applause.*)

There is a motion to adopt a resolution on the subject discussed at the session.

Are there any proposals on this point?

Academician P. N. Yakovlev has the floor. (*Academician P. N. Yakovlev reads the draft of a resolution on T. D. Lysenko's address. The reading is punctuated by frequent bursts of applause.*)

There is a motion to adopt the draft resolution as a basis and to entrust the final drafting to the Presidium. Are there any other motions? (*None.*) Allow me to put the motion to the vote. Who is in favour of adopting the resolution read by Academician P. N. Yakovlev? (*Adopted unanimously.*)

With this, allow me to close the session. (*Prolonged applause.*)

RESOLUTION

Adopted by the Session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. on the Address Delivered by T. D. Lysenko on the Situation in Biological Science

After hearing and discussing the address delivered by Academician T. D. Lysenko, President of the Lenin Academy of Agricultural Sciences of the U.S.S.R., on "The Situation in Biological Science," this session of the Academy fully approves of the address, which contains a correct analysis of the present situation in the science of biology.

Two diametrically opposite trends have become defined in biology: one trend is the progressive, materialist, *Michurin* trend, named after its founder, the distinguished Soviet naturalist and great transformer of nature I. V. Michurin; the other is the reactionary-idealistic *Weismann* (Mendel-Morgan) trend, founded by the reactionary biologists Weismann, Mendel and Morgan.

The Michurin trend proceeds from the premise that the new characters which plants and animals acquire under the influence of their conditions of life can be transmitted by inheritance. The Michurin theory arms practical workers with scientifically founded methods for the planned alteration of the nature of plants and animals, for improving existing varieties of agricultural plants and breeds of animals and creating new ones.

The Michurin trend in biology is the constructive development of Darwin's theory, a new and higher stage of materialist biology. Basing itself in its researches on I. V. Michurin's outstanding theory of the development of plants and on V. R. Williams' theory of soil formation and on his methods of creating conditions for high soil fertility, Soviet agrobiological science, further developed in the researches of T. D. Lysenko and the whole body of progressive Soviet biologists, has become a powerful instrument for the active and planned transfor-

mation of living nature. The Michurin trend in biology is day after day rendering assistance to practical socialist agriculture. It is developing a new, progressive agrobiological science which renders ever increasing assistance to the collective farms and state farms in their efforts to secure high productivity in socialist agriculture. Unity of theory and practice, which is an essential condition for knowing the laws of development of living nature, is fully and clearly embodied in the Michurin agrobiological science. Thanks to this unity, modern agrobiological science has already achieved considerable success in the scientific knowledge and control of living nature. There can be no doubt that the further development of I. V. Michurin's theory will progressively increase our successes in subjecting nature to the will of man. The overwhelming majority of researchers in the field of the agricultural sciences are following the Michurin path. These researchers must be given every assistance and support.

The Mendelist-Morganist trend in biology propounds the idealistic and metaphysical theory of Weismann that the nature of an organism is independent of its external environment, the theory of the so-called immortal "hereditary substance." The Mendelist-Morganist trend is divorced from life and its researches are practically fruitless.

This session of the Lenin Academy of Agricultural Sciences of the U.S.S.R. is of the opinion that the Michurin trend headed by Academician T. D. Lysenko has performed great and fruitful work in exposing and shattering the theoretical positions of Mendelism-Morganism. This work is of great positive importance for the development of progressive biological science and practical agriculture.

This session notes that to this day scientific research in a number of biological institutes and the teaching of genetics, plant breeding, seed cultivation, general biology and Darwinism in universities and colleges, is based on syllabuses and plans that are permeated with the ideas of Mendelism-Morganism, which is gravely prejudicial to the ideological training of our cadres. In view of this, this general meeting is of the opinion that scientific research in the field of biology must be radically reorganized and that the biological sections of the syllabuses of educational institutions must be revised.

The purpose of this reorganization must be to help to arm scientific research workers and students with the Michurin theory. This is a necessary condition for success in the work of specialists in production and in scientific research connected with urgent problems in the field of biology. Simultaneously with the revision of syllabuses, work should be organized for the issue of high quality textbooks, and of books and pamphlets to popularize Michurin's theory.

The Lenin Academy of Agricultural Sciences of the U.S.S.R. must become a genuine scientific centre for the comprehensive and deep study of Michurin's theory.

This session of the Academy is of the opinion that the researches conducted in the Academy's institutions should be subordinated to the task of assisting the collective farms, machine and tractor stations and state farms in their efforts to secure higher yields of agricultural crops and livestock produce.

This session of the Academy appeals to the body of research workers in the field of agricultural science, to all agronomists, zootechnicians and leading workers in the collective farms to rally more closely around the Lenin Academy of Agricultural Sciences of the U.S.S.R. and, under the leadership of the Party of Lenin and Stalin, and of the great leader of the working people, teacher and friend of Soviet scientists, Joseph Vissarionovich Stalin, to unite their efforts to develop Michurin's theory, the progressive agrobiological science, which is capable of fulfilling the tasks our Party and Government have set before the workers in agriculture.